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HEREDITY AND NEUROSIS.

(*Being the Presidential Address to the Neurological Society for* 1897.)

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GENTLEMEN, by your request I find myself in the most honourable position of President of the Neurological Society, and I feel in accepting this post and before entering upon my first duty, which is the giving of the Address, that I must refer to the death of Dr. Hack Tuke, which has thus placed me as the representative of the Psychiatric Branch of this Society. Dr. Hack Tuke was one of the best examples of the transmission of mental and moral characteristics from father to son. Coming of an earnest methodical family he developed, notwithstanding bad health, a power of work which was astonishing to all his friends. A seeker after truth, and a careful recorder of all he saw, he is hardly to be replaced in our branch of medicine, where the material so far exceeds the means for observing and arranging it. He has left us and I will do my best to fill his place, though I fear his mantle has not fallen on me.

The Address of last year was so masterly a demonstration of the work done by its author in the definite lines of anatomical research, that I feel my task in handling the

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indefinite and little known must make my address a poor contrast to his, but my life's work has been with those expressions of nerve power which, though disordered, may yet be made as instructive to the medical observer as are the decay and denudations of rocks to the geologist. I cannot hope to bring before you any brilliant generalisations or any new discovery. The brain anatomist and physiologist are employed in examining the basis of mental factors, and therefore we alienists must be content to wait for their work before we can hope to do more than collect, arrange and speculate.

After much consideration, I thought that my special experience might best be utilised by comparing the relationships of the neuroses, and by tracing, as far as I am able, the lineal descent of these disorders of mental function: hence "Heredity and Neurosis" is the subject I have chosen.

The subject is an old one, but with each step in advance in science old subjects are found to have new aspects. Things have as many characteristics as the observer has points of observation; there is no finality, there is no definite or ultimate knowledge. With the development of the Darwinian idea and with the full recognition of the force of evolution, immense strides have been made in all branches of human knowledge, but the goal is not yet reached. More recently Weismann has called special attention to the problems of heredity, and still later Romanes and Archibald Reid, besides innumerable foreign authors, have taken up the subject. The present time is one rather for collecting and weighing evidence than for forming a final judgment, therefore I find myself in a natural but unsatisfactory state of doubt, and I can only give my experience and my opinions, which must after all be only provisional.

Before proceeding further I will lay down briefly my belief in the part played by heredity in the development of normal and abnormal man.

We take it for granted that evolution has taken place, and we believe that it is still in progress, and that the chief factors in evolution are selection or survival and heredity. Survival of the fittest, though in the past so important a

factor, is of little value in the present phase of human development, and whether there has been marked continuous development and advance in man is still a moot point. It is quite certain that advance in the higher animals must be very slow. We watch for the appearance on the earth of new species of animals and we watch in vain, and specific continuity and invariability are arguments against any visible evolution going on at present. In human intellect we have no real evidence of advance in quantity for many ages. From the lowest savage to the highest civilised being, doubtless, there is a great advance, but even here the increase in bulk of brain is not what some might expect it to be. Men advance and are advancing, not in the mass, but in the adaptability of their nervous systems. It is the surrounding conditions which are growing, and the work of the nervous system is to adapt itself to these rapidly changing conditions. The theory at present popular is that of Weismann, which theory is, that there is a continuity, an immortality of germ plasm, and that the only real changes are in the somatic case or surroundings of this substance, or depend directly on this.

The germ plasm is looked upon as quite different and more elemental than the germ cell, it is as it were the nucleus around which the soma grows. That, just as the nucleus takes unto itself matter and forms a germ cell, so the germ plasm as a whole takes to itself a body which is specifically suited to the surrounding of the future animal. The germ plasm is constant in each species and thus it forms the fixed point about which all variations occur and it is in that we must seek for any changes if there is a transmission of acquired qualities. Weismann is supported in his contention that the germ plasm is continuous and unaltered by the fact of specific fixity; that the characteristics of the animal, *i.e.*, the specific ones, do not alter whatever may be the external or internal variations in conditions; varieties may arise depending on these changing surroundings and pass away, but the species is constant. This is apparently true as far as limited periods are concerned, but unless we believe in the individual creation of species we

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must accept the evolution of fresh species through a slow accumulation of changes which have been transmitted.

If evolution is true there must have been a gradual change in the parts which slowly aggregate to form the new species, and we have to admit that these changes, whether the result of one form of selection or another, have been sufficiently established to be passed on from parent to child. These characteristics seem to be definitely represented in some way in the reproductive elements. We no longer expect to see the oak tree in miniature in the acorn, nor the chick in the egg, we only look for a power in the seed which will enable it to grow like its parents in harmony with its surroundings. I do not intend here to consider the various theories which have been started to explain the method by which each part of an organism is reproduced in the ovum, for none of these theories are fully accepted, and none will be considered in the practical part of this paper. We accept as facts that the specific characteristics of beings are transmitted directly with a certain power of slight variation, and that the functional correlates of the transmitted organs also reappear in succeeding generations.

No one disputes that there is a direct transmission of the simpler organs of the simplest animals, nor is there any doubt about the transmission of the simpler functions such as the reflexes, and there also seems little doubt that there is a direct transmission of the parts concerned in the instincts, whether these instincts are congenitally active or only develop later with the enlarging of the surroundings. There is, therefore, little difficulty in comprehending how still more complex organic relations may be passed on, but when the connection between the organ and its function is less clear and definite, it is only natural that the mode of transmission should be more difficult to follow. It is now more fully recognised that there is no such thing as the inheritance of a fully developed function of the higher kind: the tissues and the organs reappear in succeeding generations, and from them the function may be evoked. The faculty of speech may be inherited, but the special language comes from the surrounding. Aptitude with the hands is passed on,

but the special form which the aptitude may take depends on the education. A predisposion is a transmitted, not a developed quality. It is noteworthy that among the functions which are transmitted in association with certain organs there may be temporary delay in their exhibition, and there may be a potential function in reserve which may be kept in abeyance for long periods, and yet may appear when the suitable stimulus is given. This will be seen later to have an important bearing upon certain nervous disorders, which appear only at certain stages of life, when in fact certain conditions act as the stimuli. For instance, the duckling may, in the absence of water, walk and fly and not swim, yet when the stimulus of water is present the highly evolved function of swimming is performed with exactitude and endurance.

I shall throughout take it for granted that there is no such thing as the inheritance of a function, but only the inheritance of the capacity or predisposition to react in certain definite ways under certain definite stimuli. The definite organ responds to definite stimuli, and the changed surroundings do not alter the specific character of the being.

When considering the neuroses it will be seen that the surrounding does cause their FORM to vary. This only means that in the neuroses the differences are usually not specific; they must be considered only as varieties, not species.

There is admitted to be a capacity for passing from parent to offspring special ability to react to certain stimuli, but the question which is being most debated at present is, whether it is possible to transmit directly anything which has been acquired by the individual during his lifetime. The general answer to this question is in the negative, but I think this reply has been given too definitely. For although there is no doubt that the great majority of mutilations are not inherited, yet it does not follow, because most of these coarse material effects are not passed on, that some other acquisitions or defects of a different nature may not be so transmitted.

I have already said if there is no power to vary, and no

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power to transmit the power to vary, there can be no progress, no fresh definite varieties and certainly no new species. In most living things there is a recognised power to vary, though not in the mass itself yet in its relations. There is a tendency at first, connected with the survival of the fittest to vary more and more in harmony with the changing surrounding. The power to vary and the power to transmit the power to vary, is all that is required, and I think whatever may have been one's earlier ideas, one must now accept the theory that with the higher developing surroundings it is absolutely necessary for the good of the individual that a more rapid power of reacting to the varying surroundings must be transmitted. This is the point where the inheritance of neurosis comes in. If it is accepted that with developing complexity of life there is necessarily developing complexity of relations, with a corresponding tendency to go wrong, and if there is a transmission of capacity for proper self-adjustment, so may there equally be the opposite.

Having considered the bearings of heredity, I wish to define the use I make of the terms "neurotic" and "neurosis." With development, the relationships between the *self* and the *non-self* become more and complicated, and the means of communication between the two have to be more delicate, and more unstable. The movements of a gale of wind are easily noted by a wind gauge but to mark the movements of waves of light you need an extremely delicate mechanism. The finest human adjustments can only be made by extremely mobile tissues. These delicately mobile tissues are chemically and mechanically unstable, and are sometimes too delicate for their work. Disorder may depend, not on the fineness of the organ, but rather on the force of the excitant, and it will be at once recognised that in studying nervous disorder these two conditions have to be both well weighed.

The nervous systems which are too delicate for their surroundings, and which, as a consequence, tend to be constantly upset in various ways, are called "neurotic," and the instability itself is "neurosis." It will thus be seen that

I do not mean by the term neurosis a definite disease, only a more or less definite tendency, and in my opinion it is this tendency which may be transmitted.

The connection between the so-called "insanities" and the neuroses must be studied in detail. In many cases the same nervous weakness leads to insanity on the one hand, or neurosis on the other, the result varying with the surrounding conditions of the individual and the exciting causes of the disease. Nervous diseases are not properly studied by dividing them too definitely into classes or groups; Nature is not rigid and definite in the making of living things. I trust the experimenters in psychology will be able to give us useful knowledge in reference to the "reaction times" and acuity of sense perception in the neurotic, for though there seems little ground, according to them, for believing in *types* or temperaments, yet I am inclined to think that in some neurotics, at least, there will be found some common physical sensory disorder or defect.

The neurotic individual is the natural outcome of highly specialised modes of living, and we shall find among the unstable two distinct groups—those who react very rapidly and delicately to their surroundings, and are looked upon as geniuses; and others who are unstable without being brilliant, that is, who react destructively.

The next statement I would make is that the neuroses are allied, and that this is so, will be seen in the fact that similar persons, under similar conditions, break down in different ways, *i.e.*, exhibit the different forms of neurosis; also that the same person, from similar causes, at different times, may exhibit the various forms of neurosis; and I hope to show that there is a hereditary relationship, so that in the transmission of neurosis variation in form often occurs. Much still remains to be done in tracing the causes of difference between the neuroses, and there is a like difficulty in explaining the physical origin of the nervous instability. This latter may chiefly depend on increase of bodily refinement due to habits of life, climate, and food, or it may arise from delicacy or weakness due to exhaustion or degeneracy in the parents. One is in the habit of talking of the chain

of neuroses; the simile is wanting in complete exactness, for though there are many links of this chain they are hardly to be considered as forming any connected whole. The links are of different sizes and shapes, and, though forged out of the same metal, they greatly differ. The linking is best seen by noting what may be the possible development of the neurotic type in one family. In such there may be deficient *power* or mass of brain, associated with idiocy; there may be a tendency to unequal one-sided mental development, which may be associated with precocity on one side and moral defect and criminality on the other. There may be defects of *control*, leading on the one hand to convulsions, and on the other to chorea, or to impulsive acts; there may be tendency to irregular discharge of nerve force, this leading to epilepsy, insanity of critical periods of life, and to recurrent forms of insanity. There may also be morbid forms of mental association and morbid sensory states which lead to organised systems of delusions. Besides all these, which may be seen in the same family and which may represent one another in the same person at different times, there are many other forms of nervous disorder which I shall consider in detail as I proceed. Among these other conditions we shall have to note the bodily diseases which often have a nervous aspect, or which may replace nervous diseases—such are asthma, gout, megrim, and diabetes.

Seeing, then, the varieties of nervous disorder which may be represented in a neurotic family, the study of the starting point of neurosis is all-important in the present relationship. This nervous instability, I have no doubt, depends in the majority of cases upon some bodily defect or weakness in the parent. My belief is that in these cases there is a peculiarity existing in the parent which leads to the instability in the offspring—the tendency to decay in the parent showing itself in weakness in the child. The germ plasm may be continuous, but it must vary directly in some ways with the health of the body. The germ plasm of the young animal must be taken to have more initial vigour than that of the decaying one, and the physical state resulting from bodily illness must affect the whole body. There is no possibility of one organ saying to another, "I have no need of thee."

I have seen many examples of the children of old age being neurotic, whereas previous ones were healthy; and also I have been able to trace a connection between the children begotten soon after a serious illness and those begotten before it. I have clear evidence of the instability of some children born after the parent has had an attack of insanity, the children born before the attack having been stable enough.

To put my experience briefly: neurosis or unstable nervous disposition depends in many cases on disease, or causes of bodily weakness in the parent. It is noteworthy that the conditions of nervous weakness leading to transmission of neurosis do not always exist in the insane themselves, but that an insane parent may beget or bear a normal child. The old notion that all insanity was likely to be passed on is not correct. There are very well-marked examples in which insane mothers have had several normal children, though they were insane during the whole pregnancy and after these children were born.

My belief is that physical, rather than nervous instability, is transmitted; this does not, however, cover the whole field, for in certain families, for generation after generation, the nervous balance alone seems to be very unstable, the members of these families having plenty of vitality.

The physical tendency to neurosis is easily transmitted, and in some cases a direct constitutional tendency to break down under special stresses is transmitted. Neurosis is not, as a rule, started by consanguinity alone, but if there is a tendency to nervous weakness, this is increased greatly by intermarriage.

Though an insane parent may have a normal child, there is no doubt that the offspring of the insane show their weakness in various ways, and it is here that we see so well marked the transmission of the tendency, as contrasted with the transmission of the disease.

As a rule, the children of the insane and of certain classes of neurotics resemble one another. These children may have no deficiency of brain mass, and no evidence of initial lack of any faculty, but they are *restless;* they seem to react

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unduly to all external and internal stimuli, so that there is no surplus of nervous power, no storage; memory is defective or absent, and there is no power to advance from experience. These children vary greatly in the degree of their restlessness, and also as to the time when it becomes manifest; the earlier this restlessness is marked the greater the risk of mental defect in the child.

The same defect of power of control of nervous force is seen in the various kinds of impulsive acts of these neurotic children. In some, "*rages*," or nocturnal terrors, represent undue reaction to outside stimuli. The stimuli, in some cases, are started from without the body, but in others from within, there being subjective sensory impressions or visceral impressions which, neglected by the normal child, may in the neurotic start various exaggerated reflexes. Interestingly related to these children are those who have an extreme dread of heights. Many persons, otherwise normal, have giddiness or dread on looking down great heights, but in my experience the offspring of the neurotic suffer much from this dread. Uncontrolled or exaggerated motor reflexes are seen in these children, also mimicry of a monkey-like type, and it is among such that we meet with the musical mimic, the weak-minded child with a phenomenal memory, and the calculating idiots. Such children not only react abnormally to outward stimuli, but convulsions are easily caused in them.

A skin eruption, a dental trouble, or gastro-intestinal irritation may set up fits. The occurrence of infantile convulsions does not mark every child as neurotic, but it is a very important element in the diagnoses. These convulsions occurring in neurotics may readily lead to a *habit* which becomes epilepsy, each recurring convulsion making the next more easily produced. Neurotic children exhibit various interesting disorders of expression, some of which need further study. Facial twitches are common, proceeding in some cases from mimicry, but in others from ill direction of nervous energy. Certain forms of stammering, too, are common in neurotics. Before leaving these cases, I should like to mention certain other defects of expression

(or peculiarities, I might say). In some there is a missing link between one of the sensory centres and the highest centres: thus, some cannot learn by sight, and others cannot learn by hearing, the former defect being the more common. I have seen half a dozen young patients who could not be taught to spell; they saw no relationship between alphabetical signs and sounds. Again, in studying mirror writing, I have met with idiots who did it at once when set to copy with their left hands, and I would go one step further and say that the neurotic learn to do this much more quickly than the normal; in this case, rapid power of adapting to changing conditions may mark a connection between the genius and the fool.

Defect of control seen in the neurotic may extend beyond the senses and the muscles, &c. In children belonging to nervous stock, the temperature, which is generally variable in childhood, is more erratic, the slightest bodily disorder being followed by rapid and sudden rise in temperature, which falls equally quickly. Allied to the instability of temperature in the neurotic is the facility with which delirium is started. The offspring of neurotic parents show their physical and mental instability in no way better than the ease with which they pass into delirious states. A very slight illness will be associated with nocturnal delirium, and with the simpler fevers of childhood, such as measles and the like, delirium will be present. I think this is of double interest in comparison with the easy production of convulsions under similar conditions, and the tendency in these patients later on, say at adolescence, to develop grave *delirium* or delirious mania, just as epilepsy may be the result of the convulsions.

I have met with one family in which one member had convulsions leading to epilepsy; another member died of delirious mania after measles; a third developed systematised delusional insanity, the links in the neurotic chain being easily traced to their origin, namely, a neurotic father.

The general idea that *youth* is in favour of the patient is markedly a mistake; with the neurotic, the younger the patient the greater the danger to the individual, the dangers

being either of permanent loss of power, or of permanent instability.

I hardly know where to place somnambulism in my series of derived neuroses, for, though it differs in origin in different patients, yet there is no doubt it is very common in the children of the insane and the neurotic. In some it is started during the restless state when there is undue reaction to outward stimuli; in others, it is associated with dreams, or with nocturnal hallucinations of a less pronounced type than nocturnal terrors. I have known it arise in restless, irritable children from vesical irritability. In one case which I saw recently, I have no doubt that unrecognised dreams played a chief part; for this girl recollected dreams occasionally, which were always of exactly the same type, so that the fright and reaction certainly depended on dreams in some cases, and, I believe, the similar acts were always, in this case, started by similar mental states. Automatic states will be seen to be linked with convulsive and epileptic conditions. Exaggerated irritability leads to another series of troubles—the constant wetting of the bed, and the development of precocious sexuality; in both instances I have been able to trace reflex acts as starting these morbid habits.

The descendants of neurotic parents exhibit their nervous weakness in instability of various kinds, but they also show very clearly want of power, and as might be expected in the evolution of the civilised states, the finest adjustments will be seen to fail first. In the individual the first sign of weakness as well as the first sign of decay may be a lack of adaptability; senility is often marked by rigidity and want of plasticity, and with those decaying or degenerating through defective heredity the same may be seen. In some cases the neurotic is too unstable to have definite power of adapting himself to his surroundings, and I wish now to consider such who fall under the description of the morally insane.

These classes now under consideration form the connecting links between the criminal and the insane, and a few examples will best convey my meaning. In some there is

either a want of memory or lack of power of comparing past experiences with present stimuli. Moral idiocy, moral imbecility, and moral insanity are all forms, or rather degrees, of the same thing, and it is again worthy of note that just as a previous attack of insanity may produce moral insanity in the individual, so previous insanity in the parent may leave moral insanity in the children. Of the moral defects which occur as symptoms, the most common are lying, theft, cruelty, destructiveness, and increasing precocious or perverted sexuality.

Lying may be an exaggeration of the common childish habit of "make believe." I have met with most astonishing romancers among the children of the neurotic. In these children there seemed no power to separate the imagined from the experienced, and the lying was rather a genius than a fault. On the other hand, the neurotic liar is generally malignant and cunning, and I have known such, who on the spur of the moment could invent the most elaborate lie which was hard to disprove. I daresay lying is not uncommon in its various forms among normal children, but it is necessary to consider it here in relationship to the abnormal. *Stealing* may be associated with lying (it generally is), or it may be met with alone. It again is of two markedly different types. It may be a kind of uncontrolled reflex, the mere taking anything which falls in the way of the child, or may be allied to the collectors' habit; it may be quite simple in the restless child, but may be developed by cunning through injudicious punishment. In some, punishment has no effect either in developing or checking the habit, there being either inability to remember, or what is more likely, to associate the act of stealing with the punishment. In some cases there is what one patient described to me as "lust of possession," there being recurring periods of possessive desire which might be compared to the recurring periods of sexual desire. These cases might be considered as cases of morbid impulse. The cases of destructiveness and cruelty, again, resemble those already considered in being of two classes, in the one the mere reflex desire to act leading to the destruction, the sight of fire

being quite enough to induce some of these unstable children to throw their most cherished toy into the flames. In other cases there is the exaggerated pleasure of power; this I fear, is not uncommon in the ordinary school boy, and may do little or no harm, or it may occur early in exaggerated degree in the neurotic child. The subject of sexual precocity is probably one of the most difficult to discuss fairly, yet I feel that to do it justice it is one which needs a paper to itself, and for this I am not here prepared, I have no doubt that the children of the neurotic have often very strong sexual passion developed prematurely. It is always hard to gauge the statements of sexual perverts; they are almost always anxious, like their connexions the hysterics, to make a parade of their symptoms, while they apologise for their weakness by lying, or by transferring the blame to others. I have evidence, however, that sexual self-abuse may arise in infants from local irritability—that an adherent prepuce may start a vicious habit, or that worms in the rectum, or some bladder trouble may similarly give rise to a habit which is hard to cure. This may occur in the healthy child, but is rarely so started in such. There is doubtless a very great danger to neurotic children in relation to the sexual functions, and here again we meet with the fact that similar symptoms of defective control over the desires may also occur as the result of previous attacks of insanity or of senile nervous decay. If I had to plead specially for the possibility of the direct transfer of mental symptoms from parent to child, I should certainly select cases which have been under my observation in which immoral parents have had immoral children, but in these cases, as in the alcoholic ones—in which, too, the transference of habits seems to be clear—we have many other factors to consider which show that the heredity is not clearly proved. Sexual precocity is to my mind one of the most common symptoms of neurotic origin; I have not very much experience of sexual inversion, but I believe that in these cases heredity is also supposed to play a very important part.

Already I have spent a very large amount of time on the subject of the forms of the neurosis which are met with in

the offspring of the nervous and insane; not that I expect to be able to point out the absolute physical tendency to transmission, but chiefly with the purpose of showing how a neurotic tendency shows itself in the offspring in various ways—that, in fact, there is only a transfer of a tendency which may develop in different ways according to the surroundings of the individual. Having done this, I shall now pass to other branches of my subject.

To complete the chain, heredity and the neuroses, it is necessary to refer to certain relationships which exist between bodily disease and neuroses. I cannot speak on this subject from any large personal experience, but I am inclined to think that hereditary nervous instability leads to a special predisposition to contract some contagious diseases. Sir W. Jenner pointed out that in some families there is a remarkable susceptibility; he specially referred to our royal family and its proclivity for typhoid fever. I believe that members of highly neurotic families not only take such diseases as scarlet fever, measles, typhoid, &c., readily, but that they are more liable than others to have a second attack of the same fever. If my idea is borne out and found to be true, it is only another example of the transference of a *predisposition* to unstable vitality. Micro-organisms of a dangerous nature are probably very generally present, but fortunately the soil required for their growth is not so common. I believe, however, that the neurotic subjects provide a fertile soil. If this be true of fevers, the same may hold good with phthisis. It is worth noting that this disease is commonly met with in the insane, and I believe it is still admitted that the tendency to develop phthisis is readily transmitted. Nowadays no one looks for the direct inheritance of either consumption or of cancer, but they recognise that certain families seem to be more liable to them than others. My opinion was, until quite recently, that no connection could be traced between cancer and the neuroses, but I feel now that my judgment should be reserved until I have observed further.

Next, as to the transmission of insanity, as such, from parent to child. I must make it clear that in my opinion

there are certain neurotic conditions which are more easily passed on than others. It does not follow that every neurotic must become insane, in fact there are certain forms of neurosis, such as hypochondriasis, which rarely pass beyond the border line of sanity. I think it will be best for me now to consider the part which heredity seems to play in some of the *forms* of mental disorder which are commonly recognised, not as species of disease, but as convenient groupings of symptoms. Much has been written which I have not time to discuss as to the special power of transmission of insanity which belongs to one sex or the other; doubtless the laws underlying the transmission of bodily and mental characteristics have to be studied much longer before any general rules can be formulated.

One-third of the insane are said to have neurotic heredity, but here we at once meet with a difficulty, for this makes all kinds of mental disorder of equal value in transmission. This is not so, and though I cannot pretend to weigh the exact value of each form of mental disorder as to its power of being passed on, I shall give an approximate value to each.

Mania.—In my experience, some forms of maniacal excitement are very likely to occur in the neurotic subjects, but ordinary (emotional) mania is not one of these forms. Hysteria of a grave type (with various forms of hysterical paralysis, refusal of food, malingering and the like), occurs in such persons, yet the development of these symptoms in these patients into organised mania is not common. On the other hand, I believe that neurotic subjects are specially liable to acute delirious mania, from comparatively slight causes; thus just as the neurotic child may readily pass into delirium, the neurotic adult or adolescent will easily pass into the higher grade of the same disease, *i.e.*, delirious mania. In these cases the cause is often some moral shock, some fright or sudden grief, a disappointment or an illness, which last may have started with fever and passed gradually into delirium and to acute delirious mania. It is interesting that this form of mental disorder which, in my opinion, is so frequently met with in highly neurotic families, is

characterised by extreme physical alterations, in fact the tendency is to rapid dissolution, the basis of mind being in a very unstable state.

Melancholia.—I find it much more easy, as a rule, to trace a melancholic inheritance in a family than I do to trace any other form of family neurosis, and this is interesting, for it is with melancholia that one is most accustomed to find a co-related disorder of the bodily functions. With mania you may have every bodily function apparently in order, but in melancholia you generally find all the vital processes imperfectly performed. It is, therefore, not very surprising that a bodily tendency which has a mental aspect should often be transmitted. I might go further and say that commonly many bodily ailments tend to be represented by morbid self-consciousness of a depressed or melancholic type. Visceral action when unconsciously performed is healthy action; unhealthily performed it tends to cause gloom. There is, therefore, a natural relationship between the transmission of certain unhealthy bodily conditions, and a melancholic interpretation of the morbid feelings.

I have already pointed out that in the passing on of instinct it is common to see a potentiality transmitted which may never be called into action by the surroundings; and so it is in certain families. Melancholia occurs in the members of these families when they attain advanced age, or when they are by disease brought to a premature old age. I have known such families in which, generation after generation, the members pass into melancholia, though the lives of the individuals in many instances have varied widely; some being active, some passive, some having borne the burden and heat of the day, while others rested in the shade of easy and happy circumstances, yet one and all have broken down similarly in old age. In some of the families where there has been conspicuous ability in managing affairs and in the steady pursuit of literary distinction, the end has been, in one generation after another, a melancholic shadowing of the great mental powers. I think it right also to point out here that in some of these families the characteristic instability of the neurotic is well marked.

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The brilliant ones have even during their times of work periodically to pass into the *wilderness* like the prophet of old. There are periods of deep depression alternating with periods of restless energy, and in many cases these persons end in chronic mental depression; such persons often transmit similar tendencies to recurring times of brightness and despair.

Dementia.—I need say little on this subject, but I must briefly refer to it. The natural tendency of all mental disorders is supposed to be dementia. To accept this as true one has to modify the term dementia, or use the expression "forms of mental weakness," which includes not only defect of faculties but defect of mental stability weak-mindedness and chronic and recurrent insanities being placed together. The tendency of insanities, then, is to dementia, but this is more evident in some instances than in others. The neurotic individual, though liable to insanity, is more likely to recover once or more often. Instability means tendency to go wrong, but power to be set right. The neurotic, by inheritance, then, tends to recurrent insanities with a greater chance of some recoveries, and to ultimate weak-mindedness or permanent instability. I have seen a fair number of cases in which the family tendency was always to fail first in their highest mental abilities and then slowly to pass into permanent and general dementia. Here the bodily health is often maintained to a great age, and we have the difficult task of explaining the relationship between the transmission of a healthy body with a mind tending to decay or to disorder. All I can say is that such cases are not infrequent. In dementia the lines of degeneration may follow those of development, and in generation after generation similar lines of decay may be followed. It is noteworthy that in such families one meets with tendency to apoplexy, senile epilepsy, &c., or to some visceral degeneration such as Bright's disease; one member of the family dying of one and another of some other of the diseases named.

The next subject I wish to refer to is systematised Delusional insanity, Monomania, &c. These groups are to

my mind the most interesting of all classes of insanity, and need much careful study and some generalisation to place the chaotic mass of observed facts in some sort of order. I would say the chief characteristics of the disorder are the frequency of a history of insanity in the progenitors of the sufferers, and the constancy with which most of the mental functions, apart from the delusion which dominates the mental life, remain unimpaired. It is well to recognise that in some cases of delusional insanity there is no history of insanity in the parents, but there is history or previous mental illness in the patient himself, thus once more showing the import of insanity in the individual, and its relationship to hereditarily derived neurosis.

Among the chief characteristics of the neurotic are instability or susceptibility to the influence of surroundings and the facility of forming habits, and in delusional insanity it is these characteristics which are most marked. I have met with much difficulty in explaining the apparently direct transmission in some of these cases of imperative ideas.

From the time of Darwin it has been noticed that trivial acts performed by a parent may apparently, without education, re-appear in the child—that in fact a bodily something may be transmitted which has a special mode of expression which will be transferred. The trifling, and one might say useless, variations which may occur in one generation after another may be seen in the extra digit and certain types of nose or mouth peculiar to some families. I have seen several patients whose history I have been able to examine carefully, in whom mental tricks have been transmitted from one generation to another. In one case a daughter was brought to me with the *folie de doute* and the *folie de toucher*, and I heard her father had been subject to both these peculiarities for many years. In one such case I was told the parent and child had been separated from the earliest years.

The power of transmission of muscular tricks and other peculiarities is, I think, beyond dispute. Though it is very difficult to exclude all risk of imitation, as for example, with

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the history of the transmission of special musical ability, I have met with "musical prodigies," the children of musical parents, but have never been able to decide how much was direct inheritance and how much the result of imitation; but allowing that much was due to the latter, yet there seemed to be a transmission of a greatly increased aptitude or tendency which is all one is contending for. Among the inherited tricks I should place imperative ideas which may follow neurotic heredity or neurotic illness. I have not any statistical evidence as to the inheritance of the special senses; we all recognise that deafness or shortness of sight may occur in parents and children. I am also inclined to think that I have evidence of transmission of unusual acuity of the sensory perceptions, and it is possible that with special nervous constitutions and special sensory acuity there may readily occur systematised delusional insanity. I have described elsewhere certain forms of this disorder which depend on deafness, and, therefore, I am prepared to meet with similar forms of organised delusion depending on other congenital or acquired sensory defects. Cases of delusional insanity are chiefly characterised by suspicion, doubt, ideas of plots, persecutions and the like, and there are frequently hallucinations of the senses. The memory and other faculties may be quite normal; there is little, or perhaps one should say, only a slight tendency to dementia, though there is great tendency to chronicity. I believe few of the sufferers from these organised forms of mental disorder have been free from strongly neurotic heredity, that they have been the children of old or degenerate parents, or have themselves had other attacks of acute insanity. The time at my disposal will only allow me to make this bald statement, of fact and not to enter into any more interesting points in relation to the direct or indirect transmission of sensory mental traits.

Certain other hereditary relationships deserve notice, but I fear I can only mention them in rapidly passing to my conclusion. We recognise the transmission of a tendency to develop gout, and we recognise that the disease produced by the individual himself differs little from that which may have been inherited. We recognise that

such a disease as gout may re-appear in the next generation in another form, and what is more, it may assume a neurotic character, gout replacing insanity, or insanity gout. This is not only true of gout, but of many other bodily diseases.

There is, then, a solidarity in the human body with its functions, which makes it almost a fault to speak of neurosis as a thing apart from bodily states and conditions.

I have endeavoured to trace the links existing between the bodily states and the mental expressions, and so I must leave the subject by summing up shortly as to my belief in the force of heredity and the influence of the developing surroundings in the production of nervous instability.

I have pointed out the links in the neurotic chain as seen in the offspring of the insane and of the highly neurotic.

I have shown how similar mental states may result from neurosis in a parent or from neurosis in the individual.

I have pointed out my experience as to the relative danger of transmission of the various forms of insanity; and now, gentlemen, I have completed my task, I leave it with regret, knowing how imperfectly I have performed it. Yet I leave it with the hope that, at least, I have shown you that I do not look upon heredity as a tyrant which, with inflexible power, drives the offspring to destruction. I do not think because the fathers have eaten sour grapes that all the children should have their teeth set on edge. I have striven to show that, though there is, in my opinion, power to transmit acquired peculiarities, yet the tendency is to transmit a predisposition which may be modified by surroundings.

That, in fact, it is not with the Fates we have to struggle, but with flexible powers, this justifies our treatment and encourages us to hope that we are doing our service to mankind with a prospect of doing good; hopefully we may proceed in the quest of truth, not trusting to attaining perfect truth, but having the pleasure of the quest which, like the search for the Grail, will hallow our work and consecrate our lives.

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ON HUNTINGTON'S CHOREA.

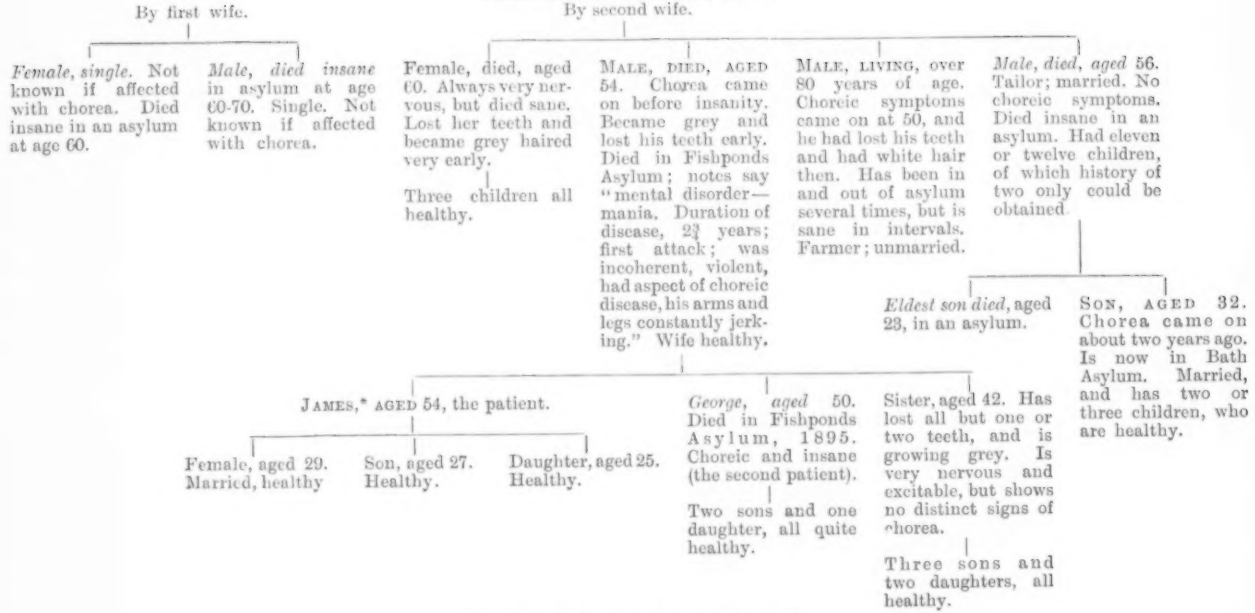
BY J. MICHELL CLARKE, M.A., M.D., F.R.C.P.

Physician to the General Hospital, and Professor of Pathology, Univ. Coll., Bristol.

THIS form of Chorea, characterised by its appearing first in adult or late adult life, its tendency to affect several members of a family, to be inherited, and to end in insanity, is now well-known, but cases are rarely met with in ordinary practice, and there have been few opportunities for *post-mortem* investigation of the morbid changes.

James T., a painter, aged 54, was admitted into the Bristol General Hospital for chorea and weakness, in September, 1895. The family history is given in the accompanying scheme. We could get no further back than the patient's grandfather, and no very definite account of him. The disease, however, appears to have come into the family through him, and not through either of his wives. On the male side there is also a history of rheumatism running through the family, and in both males and females a tendency to premature senility as shown by early greyness of the hair, and loss of the teeth is noticeable. Both the patients seen had the arcus senilis and well-marked degeneration of the arteries.

The patient was, like his brother, a small, thin man, looking older than his years, with marked arcus senilis and thickened, tortuous arteries. He had always worked hard, lived a regular and temperate life, had had no venereal disease, nor any bad illness. There was no history of any fright or great mental emotion, nor of any kind of fit or apoplectiform attack. There was no evidence of any past or present lead-poisoning. According to his wife the first symptom noticed was involuntarily stamping of the right foot five years previously; two or three months later, twitchings came on in the right and then in the left arm; soon all four limbs were affected. The patient himself said that he first noticed the twitchings on waking up one morning five years previously, and that they had persisted ever since,


GRANDFATHER OF PATIENT, JAS. T.
By first wife.
By second wife.
Female, single. Not known if affected with chorea. Died insane in an asylum at age 60.
Male, died insane in asylum at age 60-70. Single. Not known if affected with chorea.
Female, died, aged 60. Always very nervous, but died sane. Lost her teeth and became grey haired very early.
Three children all healthy.
MALE, DIED, AGED 54. Chorea came on before insanity. Became grey and lost his teeth early. Died in Fishponds Asylum; notes say "mental disorder—mania. Duration of disease, 2¾ years; first attack; was incoherent, violent, had aspect of choreic disease, his arms and legs constantly jerking." Wife healthy.
MALE, LIVING, over 80 years of age. Choreic symptoms came on at 50, and he had lost his teeth and had white hair then. Has been in and out of asylum several times, but is sane in intervals. Farmer; unmarried.
Male, died, aged 56. Tailor; married. No choreic symptoms. Died insane in an asylum. Had eleven or twelve children, of which history of two only could be obtained.
Eldest son died, aged 23, in an asylum.
SON, AGED 32. Chorea came on about two years ago. Is now in Bath Asylum. Married, and has two or three children, who are healthy.
JAMES,* AGED 54, the patient.
Female, aged 29. Married, healthy
Son, aged 27. Healthy.
Daughter, aged 25. Healthy.
George, aged 50. Died in Fishponds Asylum, 1895. Choreic and insane (the second patient).
Two sons and one daughter, all quite healthy.
Sister, aged 42. Has lost all but one or two teeth, and is growing grey. Is very nervous and excitable, but shows no distinct signs of chorea.
Three sons and two daughters, all healthy.

* A twin brother died when two days old.

gradually growing worse. During the last six months he had been less intelligent, his memory had been failing, and his manner was changed. For two nights before he was admitted his wife had to sit up all night with him, as he was in a very excited state, looking wild, and talking constantly and incoherently. After admission he was quite quiet.

State on Admission.—Abdominal and thoracic viscera normal. No cardiac murmurs; heart sounds being normal. Urine normal. Constant choreic movements affect all the muscles of the trunk and limbs; walking is difficult on account of them, but though the knees are often suddenly flexed, the spasms are not strong enough to throw him down. The facial muscles are affected, the patient making grimaces from time to time; the tongue can be protruded, but is jerked backwards and forwards. The ocular muscles are unaffected. During voluntary movement the chorea becomes worse, and the hands and arms especially are agitated by spasmodic jerking movements when he tries to do anything with them. He can, however, feed himself if his food is cut up for him. Though voluntary actions are thus interfered with they are not distinctly ataxic. Muscular power rather weak; dynamometer, 22 kilos., both for right and left hands. The muscles are small but show no wasting. Electrical reactions normal. The choreiform movements are constant during the waking hours, but cease entirely when he is asleep.

Eyes.—Pupils equal, of normal size, react well to light and accommodation. There was marked error of refraction; on ophthalmoscopic examination the right optic disc was pale, with mottling of choroid on nasal side (Guttate choroiditis). Left optic disc not so pale, choroidal changes were the same as in the right eye.

Superficial reflexes active, a well-marked "goose-skin reflex" is obtained over the skin of the chest and abdomen. Knee-jerks normal; no ankle clonus; front tap contraction present; no muscular rigidity. Sensation to touch and pain normal, and power of localising sensations fairly good, but very slightly deficient over the hands. No affection of smell, taste or hearing. The functions of micturition and defæcation are natural. His face is somewhat expressionless, and he does not succeed in frowning or expressing surprise. He answers simple questions well, but if they are at all complicated he answers irrationally. He often repeats his statements. He has no delusions or hallucinations. His memory is defective and inaccurate. He sleeps

well and has a good appetite. When in bed he can move his feet and legs well, and to any desired position, and there is no loss of the sense of position. The muscular sense appeared to be quite normal. He improved in nutrition during his stay in the hospital, but otherwise his condition was unchanged. He was given liq. arsenicalis in gradually increasing doses.

The younger brother of the above patient, George T., was in the Bristol Lunatic Asylum at the same time. I am greatly indebted to Dr. J. V. Blachford, Assistant Medical Officer to the Asylum, for notes of the case and of the *post-mortem* examination, and for the opportunity of seeing the patient and making the microscopical examination of the central nervous system. He was a small, thin, grey-haired man, who had for several years suffered from choreic movements affecting the limbs and trunk. He had never had syphilis. The diaphragm was unaffected, and he had no difficulty in swallowing. For three months previous to admission into the Asylum on July 1, 1893, he was irritable and dangerous, and for twelve months his memory had been failing. He was demented, and had the delusion that the attendants were keeping back money that had been sent to him, but apart from this delusion he was quiet, and showed some amount of intelligence and power of memory. He answered questions as to his past life readily and correctly. The thoracic and abdominal viscera were normal; the heart being unaffected. The skin showed a condition of mild ichthyosis, best marked over the abdomen and thighs. The muscles though small showed no decided wasting. The knee-jerks were somewhat exaggerated, but there was no ankle-clonus nor muscular rigidity. The pupils were equal and reacted well, the ocular movements were unaffected by spasm. He protruded his tongue spasmodically, and after much effort, and he walked with some difficulty on account of the muscular spasm. It is unnecessary to describe the choreiform movements, which were constant, and affected the muscles of the face, trunk and limbs, as they were precisely similar to those above described in the elder brother's case. They ceased entirely during sleep. In both cases the spasmodic movements were exactly like those of ordinary chorea. This patient died in the Asylum, after a few days' illness, of pneumonia, in November, 1895. The *post-mortem examination* was made twenty-seven hours after death by Dr. J. V. Blachford. There was lordosis of the lumbar spine. The *skull cap* showed a slight degree of transparency all over with some thinning at bregma. Pacchionian depressions well-marked. Diploë dense and scanty.

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Measurements:

Average thickness		45 cm.
Diameters (Internal)	Antero-posterior	17 cm.
	Transverse ...	13 cm.
Circumferences ...	Antero-posterior	31·2 cm.
	Transverse ...	31·2 cm.
Horizontal		52 cm.

Except for some injection of the pia-arachnoid the membranes were normal, and the pia-arachnoid stripped easily, and was not adherent to the cortex. The gyri appeared normal. The grey matter over the motor area was well differentiated into a light intermediate band between two darker bands. The ventricles contained some excess of cerebro-spinal fluid. No abnormal changes were visible to the naked eye in the basal ganglia, pons, medulla, cerebellum, or spinal cord.

The whole brain weighed 44 oz., the pons, medulla and cerebellum, 6 oz. The xiphoid cartilage of the sternum was bifid. The *right lung* weighed 44½ oz.; the lower lobe was in a state of red hepatization, portions sinking in water, the rest of the lung was intensely congested and œdematous. The *left lung* presented some fibrous contractions at the apex; there was some general congestion and œdema. The pleural cavities were obliterated by general adhesions. The other thoracic and abdominal viscera presented no morbid changes. The brain and spinal cord were hardened in Müller's fluid.

The following table gives the average of numerous measurements of the grey matter taken from the convexity of the convolutions in the several lobes of both cerebral hemispheres, after hardening about three months in Müller's fluid, together with those of a case reported by Dr. Charles L. Dana, and of the normal grey matter as given by him in his paper.[1]

The measurements are in millimetres.

	This case		Dana's case	Normal
Frontal lobe ...	... 2·50		2·85	3·1
Motor convolutions ...	2·56	Superior central ...	2·2	2·75
		Parietal lobule ...	2·2	
		Third central ...	1·9	
		Lower central ...	2·2	3·0
Temporal lobe ...	2·75		3·2 and 3·1	3·1
Occipital lobe ...	2·58		2·1	2·6 to 2·5

[1] *Journal of Nervous and Mental Disease*, vol. xx., p. 577.

Sections of the fresh cerebral cortex were cut and stained by Bevan Lewis's method; and of the hardened brain from all parts of the cerebral cortex and from the cerebellum, after embedding in celloidin, were stained in hæmatoxylin and eosin, methylene blue, aniline blue-black, hæmatoxylin and picro-fuchsin (Van Giesen), and by Weigert's method.

Pia-mater.—Vessels full, especially over the frontal and motor convolutions, their walls were, as a rule, normal, but, in a few instances, slightly thickened.

Convolutions of frontal lobe; vessels prominent, their walls healthy, and the perivascular spaces distended; in superficial layers of the cortex there were a few microscopic hæmorrhagic extravasations. The layer of small granule cells was narrow and stained badly. The small pyramidal cells showed morbid changes, many of them appearing shrunken, others deeply pigmented, their nuclei obscure and processes stunted. The large pyramidal cells and branched spindle or multipolar cells of the fifth layer appeared healthy.

In the motor convolutions the vessel-walls were healthy, but here and there a few red blood cells had escaped from the vessels. In a very few places the perivascular sheaths contained numerous leucocytes, which were passing into surrounding tissues. The small pyramidal cells showed the same changes as in the frontal region; a few of the large pyramidal cells contained much pigment obscuring the nucleus, but they were mostly normal. Cells of fifth layer normal. In both frontal and motor regions the number of small branched glia or interstitial cells seemed to be excessive.

In the occipital and temporo-sphenoidal lobes similar microscopic changes, but in less degree, were observed; in the former the glia cells were especially conspicuous, and in a few places wandering cells were present in the pericellular spaces around the nerve cells. Sections of the cerebellum, prepared by the above methods, showed injection of the small vessels of the cerebellar cortex, but otherwise presented no abnormal changes. The fibres in the white core of the convolutions, the cells of Purkinje and of the granule layer, appeared healthy. Unfortunately the nitrate of silver process was for some reason, a failure in the cerebellum.

Sections were also prepared according to Dr. Berkeley's modification of Golgi's nitrate of silver method (BRAIN, vol. xviii., p. 473). This gave good results, with the drawback that in some cases in cutting the sections the cell processes became broken across by the microtome knife, and also, like all the nitrate

of silver processes, the staining is very capricious, only a proportion of the cells being stained; larger or smaller, as the case may be. Further, we do not know whether normal or abnormal cells best take the stain; so that so far as proportion of normal to abnormal cells is concerned, no conclusions can be drawn from the sections, but it was quite evident that there were a large number of morbid cells, and this stain also brought out the fact, observed in sections stained by the other methods, that the change is a partial one, healthy cells lying side by side with diseased ones. The most superficial layer of the cortex was healthy, the second layer stained very badly in all sections, and appeared narrow; those cells that were stained mostly appeared normal. The small pyramidal cells, as in the other sections, showed the most marked changes, many being shrunken, irregular in shape, their processes stunted with small nodose swellings upon them, and disappearance of the little "gemmulæ" in the affected processes. No such swellings were seen, however, upon the axis cylinder processes, when these were visible. The large pyramidal cells, and the cells of the fifth layer, showed much less marked changes, being, where stained, with some few exceptions, healthy. The nerve fibres of the white and grey matter and collaterals stained well and appeared healthy, both by the nitrate of silver and by Weigert's method. The glia cells were conspicuous in all parts of the cortex, especially in the frontal and motor regions, from their great number and large size, and appeared in many instances to be attached to pyramidal nerve cells, lying in immediate proximity to them. The occipital cortex was also rich in large, fine and richly-branched glia cells in its deep layer and subjacent stratum of white matter. In the parts of the cortex most affected by the morbid changes, the processes of the glia cells showed little nodose enlargements upon them, which I have not before observed. The pons and medulla appeared to be healthy. Spinal cord: grey matter in all parts normal; nerve cells in all parts healthy. The pia matter was thickened, and there was a slight increase of the processes running into the cord from it, and of the neuroglia throughout the cord. No system-degenerations were present, the white matter being otherwise healthy.

It should have been mentioned above that the fibres in the white centre of the cerebral convolutions appeared healthy, the vessels in parts, but not universally, much injected. The glia cells were conspicuous and abundant throughout all sections. No further changes were noted in the white matter.

To sum up, the morbid change consisted in a widespread but partial degeneration of the cells of the cerebral cortex, especially the cells of the second and third layer, most marked in the frontal and motor convolutions, together with an increased amount of interstitial tissue and number of neuroglia cells.

In this family males have been chiefly affected. It will be noticed that the first patient showed evidence of mental affection; and that in the case of the sons of the patient's youngest uncle, both insanity and chorea have come on at an earlier age than in the others. According to Diller and Suckling, the disease comes at an earlier age in each generation, but this is by no means always the case. Wharton Sinkler (*Journal of Nervous and Mental Disease*, 1889) states that the disease may skip a generation.

Another case of this disease was under my care in the Bristol Hospital in 1893. In this patient, a compositor, 46 years of age, the family history could not be fully obtained, but so far as could be ascertained, the tendency to insanity was less marked. His father was affected with choreiform movements, which came on two or three years before his death at 42 years of age; he was a temperate man, and was killed by falling from the roof of a house; the spasmodic affection of the muscles was said to have been the cause of the fall. The patient had two brothers and two sisters alive and well; two other sisters are affected with choreiform movements, one of the latter is in the workhouse, and is said to be crazy: her children are mentally defective, the Board-school teachers finding it impossible to teach them. There is no other case of insanity in the family. The patient himself used to drink at one time, but had been a teetotaller for twelve years. The illness began with loss of power in the legs, then the twitchings came on, and soon extended to all parts of the body. He also suffered from dyspepsia, and occasionally from attacks of frequent micturition. He was grey, poorly nourished, and looked ten years older than his age. The thoracic and abdominal organs were healthy. Heart of normal size, and the sounds normal. His face was rather expressionless; he spoke slowly and indistinctly, with a

somewhat slurred utterance. Intelligence defective; memory very bad, and inaccurate; he showed a marked want of power of sustained attention, and answered questions badly, wandering off to other subjects.

The muscular system generally was only moderately well developed; there was no wasting, the muscles being well nourished, but flabby and weak. No rigidity, nor excessive muscular irritability to percussion. The dynamometer gave 21 kilos. right, and the same left hand. No fibrillary tremor. No paralysis. The muscles of head, neck, arms, and legs, and to less extent those of trunk, were agitated by constant spasms, in their general characters exactly resembling those of ordinary chorea, but rather more sudden and shock-like. These choreiform movements ceased during sleep, were aggravated by movement, or by talking; when lying down his head was occasionally suddenly raised up from the pillow. The movements interfere with walking, and have caused him to fall down. The facial muscles are affected, and those of the eyelids, but not of the eyeball. He protrudes the tongue well, and has no difficulty in swallowing. The respiratory movements were occasionally noted to be a little irregular. There is no in-cordination of movement.

The special senses were unaffected; sensation of all forms was normal, except that there was some loss of the temperature sense over the feet. Knee-jerks normal. Superficial reflexes; cremasteric and right abdominal not obtained, others present, plantar exaggerated. Pupils normal, equal in size, react well to light and accommodation. The temperature was constantly sub-normal, 97° to 97·5°. The bowels acted regularly.

This patient's daughter, aged 14, was said by him to be suffering from the same disease; but I saw her, and found that she was affected with diplegia and athetosis, most marked on the right side of the body, and the result of injury to the brain during birth. This bears out an observation in the *Lancet* (December 21, 1895), "that this is almost invariably the actual condition in cases of so-called congenital chorea, so that the term is probably a misnomer, and the movements would be more correctly described as those of athetosis."

In a paper by Dr. E. S. Reynolds (*Med. Chron.*, vol. xvi., p. 21) will be found an excellent account of several cases, with references to the history and literature of the subject.

Important contributions to the study of the pathological anatomy of the disease have been made recently. Menzies (*Jour. Mental Science*, October, 1892, and January, 1893) describes two families: in the first, 25 persons out of 100 traced were affected, the average age of onset was 27·6 years, and of death 43·7 years. In the second family: 13 persons out of 74 suffered, the average age of onset being 37·2 years, and of death 54 years. He concludes that the disease may descend from either parent to either sex, but that males are more commonly affected.

The *post-mortem* changes in the brain were slight coarseness of neuroglia, thickening of vessels, cell-degeneration, and, in hardened specimens, a few spider cells; in the cord, a diminution in the cells of Clarke's column, slight sclerosis of the ascending antero-lateral tract in all parts, and sclerosis of the posterior columns in the cervical region.

Oppenheim (*Centralb. f. innere Medicin*, 1894, p. 918) reports two cases: (1) a man who died of apoplexy, aged 75, in whom the disease had lasted sixteen years; and (2) a woman, who died of influenza, aged 56, in whom the duration was five years. There was a history of heredity, but I find no mention of insanity. *Post-mortem*, the heart was healthy in each case. In the brain the gyri were narrowed, and the sulci broadened in the motor, parietal and occipital regions; in these regions there were foci of hæmorrhagic infiltration in recent, fibrillar in later, stages in the sub-cortical layer. In the cortex, the small round cells next below the uppermost layer were deficient in number. The pyramidal (? large) cells were normal. The basal ganglia were normal. In the cord there was increase in the neuroglia cells and fibres, chiefly in the lateral columns; these changes he does not regard as secondary to the cerebral lesions, as they are not systematic, and further, affect only the neuroglia and vessels. The peripheral nerves examined were found degenerated; and this he attributes in the first case to old age, in the second to influenza. He thinks the essential morbid con-

dition to be a miliary disseminated encephalitis, cortical and sub-cortical, followed by atrophy of the cortex.

Dr. Charles L. Dana's (*Jour. of Nervous and Mental Disease*, vol. xx., p. 565) patient was a man in whom symptoms first appeared at the age of 33, and who died of typhoid fever at 37. The case constituted a transmission through females to the fifth generation. The convolutions of the brain showed anomalies in an interruption of the fissure of Rolando, and absence of the superior "*pli de passage.*" There was a general thinning of the grey matter, most marked over the central convolutions; areas of cell-degeneration, the angular and small pyramidal layers being most affected, the cell-defect being primary, the vascular changes secondary. He considers the disease belongs to teratology, being an innate defect in cell structure.

Kronthal and Kalischer (Virchow's *Archiv.*, Bd., cxxxix., Abs. in *Neurol. Centralb.*, May 15, 1895), on the basis of the examination of three cases of chronic hereditary progressive chorea, conclude that the morbid process in Huntington's chorea consists in diffuse, rarely circumscribed, changes in the cortex of the brain; these are essentially disease of the vessel walls, increase of nuclei, cell accumulations, small hæmorrhages, and increase of the interstitial framework; whilst the nervous elements are only slightly affected.

There seems then to be no doubt that the cerebral cortex, especially of the motor convolutions, is the seat of disease; the chief difference of opinion being as to whether the primary change is in the nerve cells themselves or in the supporting tissue. So far as my own sections go they point to a degeneration of the nerve cells, with a concomitant increase of the neuroglia.

Changes of a similar nature to those above described, but less marked, have been observed in cases of severe chorea of the ordinary type. Severe cases of ordinary chorea occasionally end in mania. In a girl, who suffered from an acute attack of extremely severe chorea, which became complicated with acute mania, and who died from exhaustion, I found, *post-mortem*, intense congestion of the small arteries and capillaries of the cerebral cortex. The bright injection

of the vessels was obvious to the naked eye, and, under the microscope, sections of the cortex showed injected capillaries in extraordinary abundance, with some minute hæmorrhagic extravasations and cell-exudation, most marked over the motor area. The vessels of the basal ganglia and pons were also injected, but in much less degree.

The identical character of the spasms in cases of Huntington's chorea, and of ordinary chorea, points to affection of the same part or parts of the central nervous system in both cases. The severity, long duration and intractability of the symptoms in the former should render the recognition of the underlying lesion more easy, because the morbid changes would naturally be more extensive. The *post-mortem* changes above described indicate the cerebral cortex, especially of the motor convolutions, as the seat of the morbid change. The conclusion seems to be that all forms of chorea are due to disturbance in this region of the brain, although in each form the exciting cause of this disturbance is probably of a different nature, and the precise nature of the resulting alterations and capability of recovery vary accordingly.

In conclusion, I must express my best thanks to Mr. James Taylor for his kindness in making many excellent photographs of my sections, and to my clinical clerk, Mr. C. P. Mackie, for taking much trouble in investigating the history of the family above recorded.

EXPLANATION OF PHOTOGRAPHS.

FIG. 1.—Frontal cortex, low-power magnification; shows nearly whole thickness.

Degenerated and healthy cells in second and third layers; there are several degenerated cells to the left of the figure. Glia cells.

FIG. 2.—Motor cortex, low power. The surface of cortex lies to the left and above.

In the pyramidal cells of the second and third layers, some are healthy, others swollen, rounded, and irregular in outline. In uppermost part of figure is seen a glia cell attached to degenerating small pyramidal cell (*a*); above this, again, is a healthy pyramidal cell.

(In some cases the main process of the cell is artificially separated with a sharp fracture by the microtome knife.)

FIG. 3.—Motor cortex, high power. Degenerating pyramidal cells; the one (*a*) on the left hand shows swellings on main processes, and absence of "gemmulæ," and the cell body is deformed. (The process itself shows fractures caused by the knife.)

FIG. 4.—Motor cortex, high power. One or two healthy and other degenerate pyramidal cells. (Processes of some cells have again been broken in cutting.) (*a*) Healthy cell with neuron.

FIG. 5.—Motor cortex, high power. Glia cells, with small nodosities on their branches.

FIG. 6.—Occipital cortex, high power. Group of large, richly-branched glia cells in subcortical layer.

NOTE.—*The first two photographs should be viewed with a hand-lens.*

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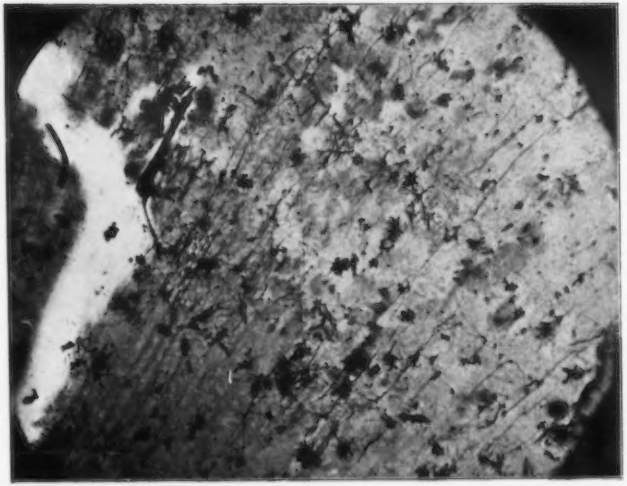


FIG. 1.

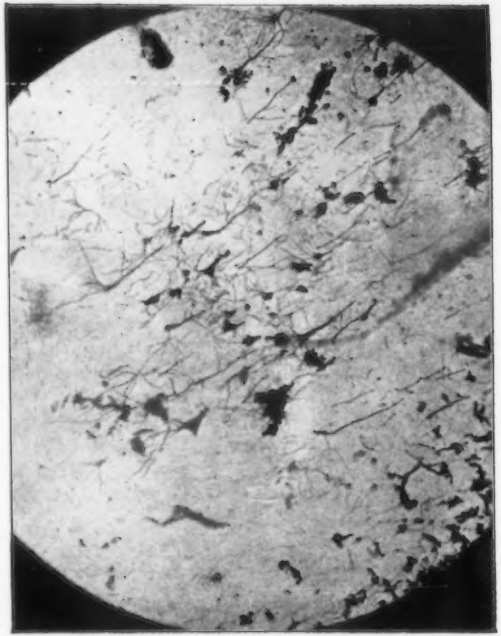


FIG. 2.

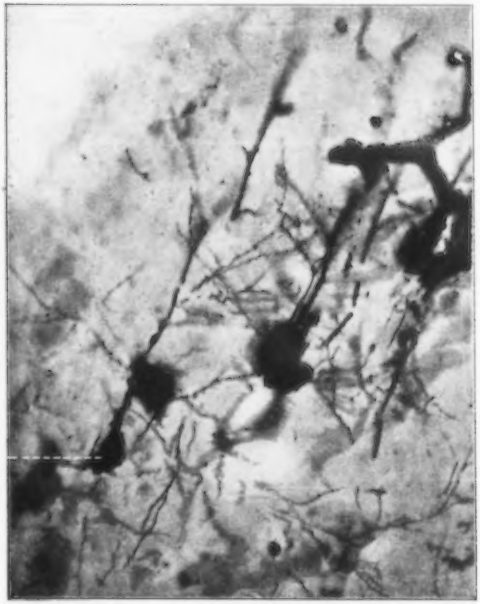


FIG. 3.

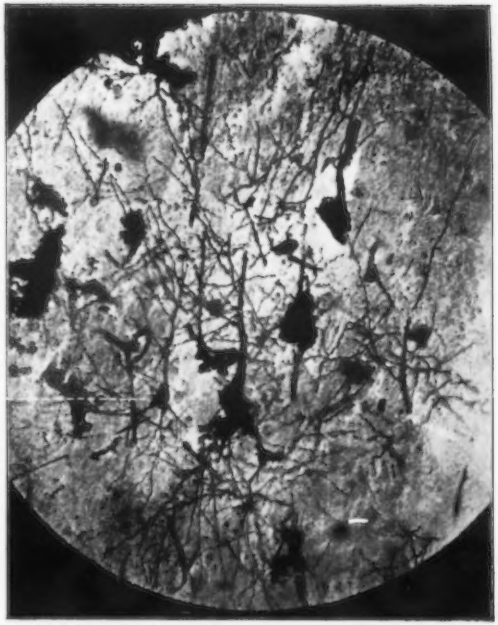


FIG. 4.

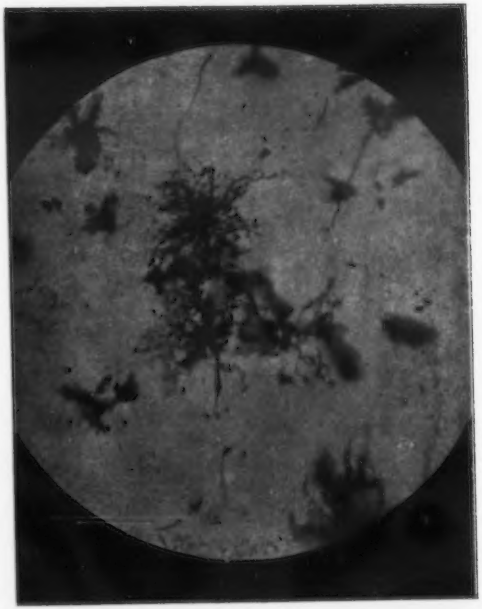


FIG. 5.

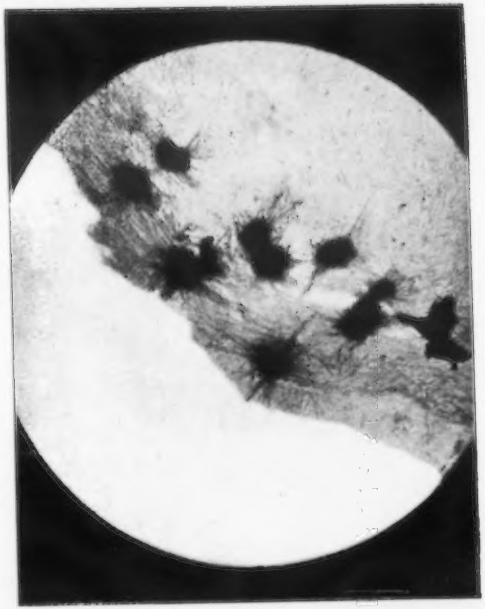
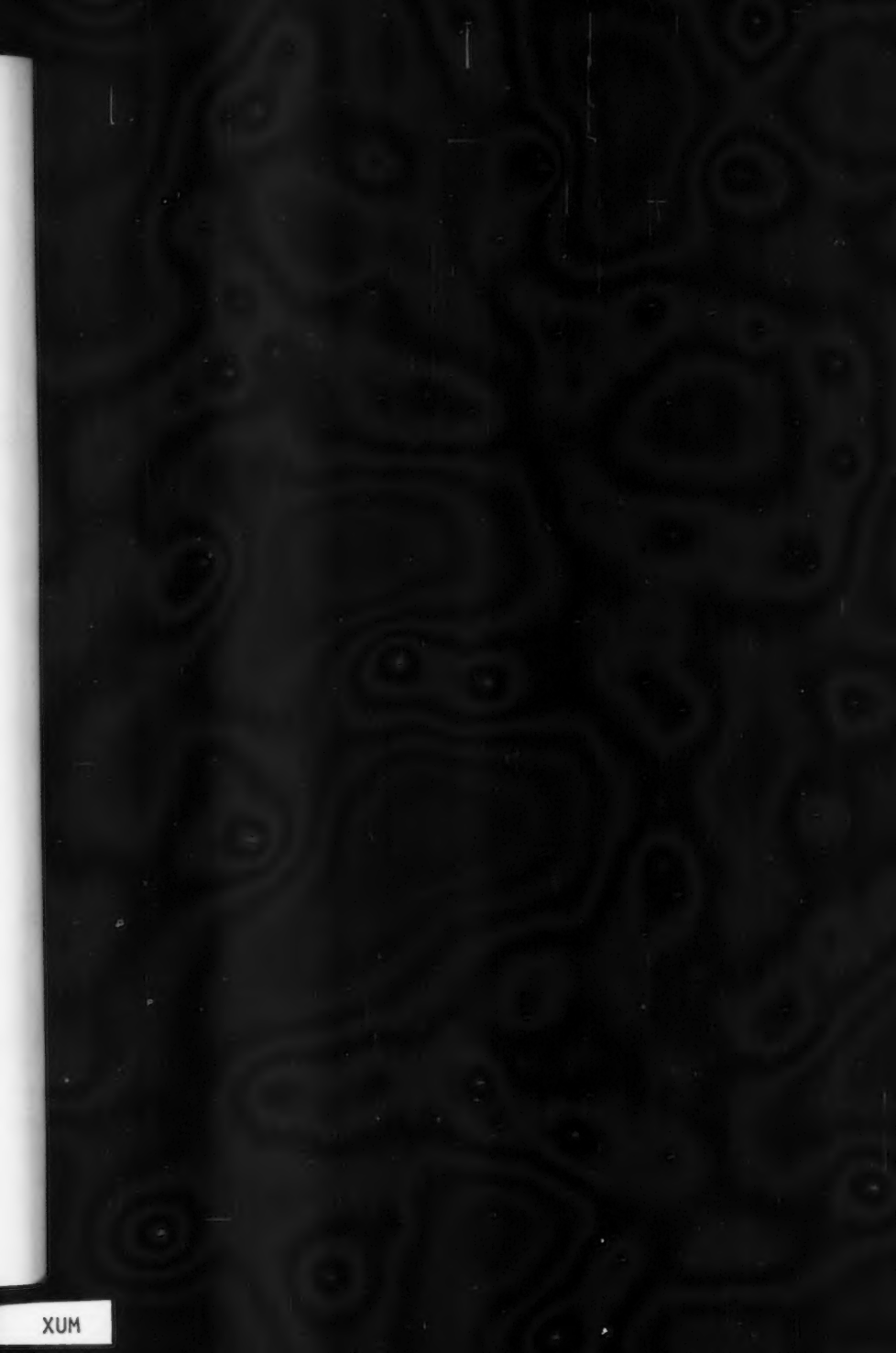


FIG. 6.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AN EXPERIMENTAL INVESTIGATION OF THE CERVICAL AND THORACIC NERVE ROOTS IN RELATION TO THE SUBJECT OF WRY-NECK.

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INTRODUCTION.

I AM indebted to Professor Victor Horsley for allowing me to carry out the majority of the experiments on which this paper is based in the pathological laboratory of University College, London, when it was under his directorship. I have also to thank Professor Vaughan Harley for enabling me to perform a few supplementary experiments in the Laboratory for Pathological Chemistry. To Dr. Max Lowenthal my sincere thanks are due for his kind assistance in some of the latter experiments; his help was especially valuable to me when I was checking the results which I obtained in connection with the anterior neck muscles.

There is an abundance of evidence to show that in a large proportion of cases of spasmodic wry-neck, surgical measures, such as division of muscles and stretching of the spinal accessory nerve, are useless, and that there must be few cases in which even excision of a portion of the spinal accessory nerve can be expected to lead to permanently good results, in that, in the majority of cases which come under observation, muscles beyond the control of this nerve take part in the spasm. Further, in that the sterno-mastoid has a double nerve supply, the spinal accessory and the posterior branches of certain cervical spinal nerve roots, even in those cases in which the spasm appears to be limited to this

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muscle, it scarcely seems reasonable to suppose that severance of only one of its paths of connection with the central nervous system is likely to do more than lessen the spasm in the muscle, while another path by which impulses can reach it from the nerve centres is still intact. Complete section of both of these paths of connection with the central nervous system can alone be expected to result in complete arrest of spasm in the muscle. If this be true with regard to the cases in which the sterno-mastoid alone appears to be involved, how much more unlikely is it that any such surgical measures directed to the spinal accessory nerve can influence the spasm in muscles over which this nerve has no control, and which are obviously involved in so many cases of wry-neck.

The only surgical measure which offers a reasonable prospect of permanent relief in such cases is division (with excision of portions) of the posterior branches of the upper cervical spinal nerve roots, an operation devised by Gardner in Melbourne and Keen in America, and which has been practised by Horsley in this country. This being the case, it seems of no small importance that we should accurately determine by experiment: (1) The exact number of nerve roots, electrical excitation of which results in contraction of muscles which bring about movements of the head on the trunk; (2) the precise position in which the head is placed by contraction of the group of muscles supplied by each individual nerve root; (3) in how many roots a given muscle is represented; and (4) the muscles represented in each nerve root.

The necessity for such an investigation will be obvious if the results which I have obtained in the present research are compared with the statements made in the standard text books on anatomy in this and other countries; and, in order to emphasize this, I have drawn up two tables in which my results are compared with the statements made in the last edition of Quain's "Anatomy."

While many observers have investigated the nerve roots which innervate the limbs, with a view to ascertaining the movements and muscles represented in each root as far as I

am aware, no similar attempt has been made to determine the movements or muscles represented in the upper cervical roots; roots concerned with the movements of the head on the trunk. The only exception to this statement is to be found in an investigation carried out by Beevor and Horsley,[1] which had as its object, the determination of the function of certain cranial nerves, in addition to which the three upper cervical nerve roots were stimulated with a view to ascertain the root supply of the sterno-hyoid, sterno-thyroid, and omo-hyoid muscles.

The experiments on which the present paper is based have been conducted in monkeys, *Macacus rhesus* chiefly, and also a few *Macacus sinicus*. The animals were always anæsthetised by means of ether administered by inhalation; they were kept under the influence of this anæsthetic throughout the course of the experiment, and killed by an overdose of it at its termination. During the operative procedure necessary to expose the cervical spines and neural arches, great care was taken to keep to the middle line as far as possible, and not to sever more of the attachments of the muscles than was absolutely necessary to allow the nerve roots to be satisfactorily exposed in the neural canal by removal of the cervical spines and arches. After effecting an entrance into the neural canal the dura mater was opened, and all the posterior roots of the cervical series were divided on one side, together with a varying number of the thoracic posterior roots, in different experiments, and also the roots of the spinal accessory nerve.

Owing to the fact that the intra neural portions of the upper cervical motor roots are so short, they were left intact for excitation, and were not divided, except for purposes of control in some instances. As this plan is open to the objection that many of the results obtained might be due to spread of current along the cord, &c., to roots other than that excited at any given instant, every possible means was taken to avoid error from this source by comparing the results of excitation of contiguous roots, of intermediate portions of

[1] Beevor and Horsley, "Proc. Roy. Soc.," 1888, vol. 44, p. 269.

the spinal cord, of the dura mater contiguous to the root excited, and so on. But, of course, the most reliable control experiments were those in which these motor roots were severed from the spinal cord before their peripheral ends were stimulated.

Excitation was effected by fine, closely approximated platinum electrodes attached to the secondary coil of a Du Bois Raymond's inductorium, supplied by a single bichromate cell. The strength of current employed was 30 or less, as measured on Kronecker's scale, which corresponds to an interval of about 27 c.m. between the primary and secondary coils. Such a current was always sufficient to evoke a well-marked response, and did not cause any trouble by spreading to parts other than the root to which the electrodes were actually applied. In all the experiments the first point investigated was the position assumed by the head on excitation of the motor portions of the individual cervical and thoracic roots. The investigation was further conducted so that a given root was excited in some instances and all the muscles which responded were noted, while in other instances a given muscle was kept under observation, while each nerve root of the series investigated was in turn excited, and a note made of those, stimulation of which evoked contraction in the muscle.

Movements of the Head resulting on Excitation of the Anterior Spinal Nerve Roots.

Despite the fact that the precise nerve roots in which a given muscle was represented varied somewhat in different animals, the position assumed by the head on excitation of the different roots remained wonderfully constant, exceedingly slight shades of difference being occasionally detectable in some animals as compared with others, but the predominant movement was always the same for any given root in all the animals investigated. The cervical roots further showed a wonderfully constant paired arrangement as to function, as far as the head movements were concerned, the first two being responsible for a very similar movement, whilst the third

and fourth were responsible for a movement which differed from that observed in the first two roots, but agreed closely with each other ; the fifth and sixth caused little movement of the head on excitation, while the seventh and eighth caused a more decided movement, the character of which was very similar in the two roots.

First Cervical Root.—The movement characteristic of an excitation of this root is a well-marked lateral inclination of the head on to the shoulder of the side excited, so that the side of the head and face are approximated to the shoulder. This is a comparatively pure lateral flexion movement, in which there is next to no backward movement, or rotation of the head about the vertical axis of the body, the chin, however, pointing to the opposite side from that stimulated. (See fig. 1.)[1]

Second Cervical Root.—Excitation of this root produces a movement of the head closely resembling that which resulted on excitation of the first root, *i.e.*, a lateral inclination of the head to the side stimulated, by which means the side of the head and face are approximated to the shoulder. As a rule, there is added to this a slight backward movement of the head to the side stimulated, which is accompanied by the slightest possible rotation of the head on its vertical axis, and the chin inclines to the opposite side to that of the root excited. (See fig. 1.)

Third Cervical Root.—Whereas the lateral movement of the head, by which the side of the head and face are approximated, is the predominant movement represented in the first and second roots, the third root is responsible for a movement in which, though the lateral movement is still evident, the movement which predominates is distinctly one in which the head is drawn backwards, and, the occiput being drawn to the side excited, the chin is directed upwards and to the opposite side. (See fig. 2.)

Fourth Cervical Root.—All that has been said with regard to the movements represented in the third root is applicable to the root now under consideration, the only difference

[1] The figures are from instantaneous photographs taken for me by Dr. Worrall at the moment of excitation of the second and fourth roots respectively.

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between the two roots being that in the fourth there is still less of the lateral inclination of the head so characteristic of the first and second roots, while the backward movement of the head, in which the occiput is drawn to the side stimulated, is still more pronounced than in the third root. (See fig. 2.)

Fifth Cervical Root.—In striking contrast to the marked movements of the head noted in connection with excitation of the upper four cervical nerve roots is the exceedingly feeble movement of the head which results on excitation of this root. The little movement that results consists in a slight drawing of the occiput backwards, and to the side stimulated. The degree of this movement varies more than that of the movements represented in the other roots, sometimes being very indistinct and at other times leaving no question in the mind of the observer as to its existence.

Sixth Cervical Root.—The movement represented in this root corresponds, in all respects, to that represented in the fifth, and calls for no further comment than that already offered in connection with the fifth root.

Seventh Cervical Root.—While much less than the movement produced on excitation of the first four cervical roots, the movement which results on excitation of this root is decidedly more pronounced than that observed in connection with the fifth and sixth roots. It consists in a drawing of the occiput slightly backwards to the side stimulated, and is obviously a secondary, or indirect, movement caused by the pull exerted on those muscles attached to the head and scapula, by the vigorous contraction of the latissimus dorsi, which pulls the scapula downwards.

Eighth Cervical Root.—Very similar in all its details is the movement of the head produced by stimulation of the motor portion of this root. As in the case of the seventh, movement of the head appeared to be entirely one indirectly induced by the vigorous drawing down of the scapula by the latissimus dorsi.

First Thoracic Root.—The movement of the head obtained on excitation of this root depends largely on whether the latissimus dorsi is represented in it or not; when it is so

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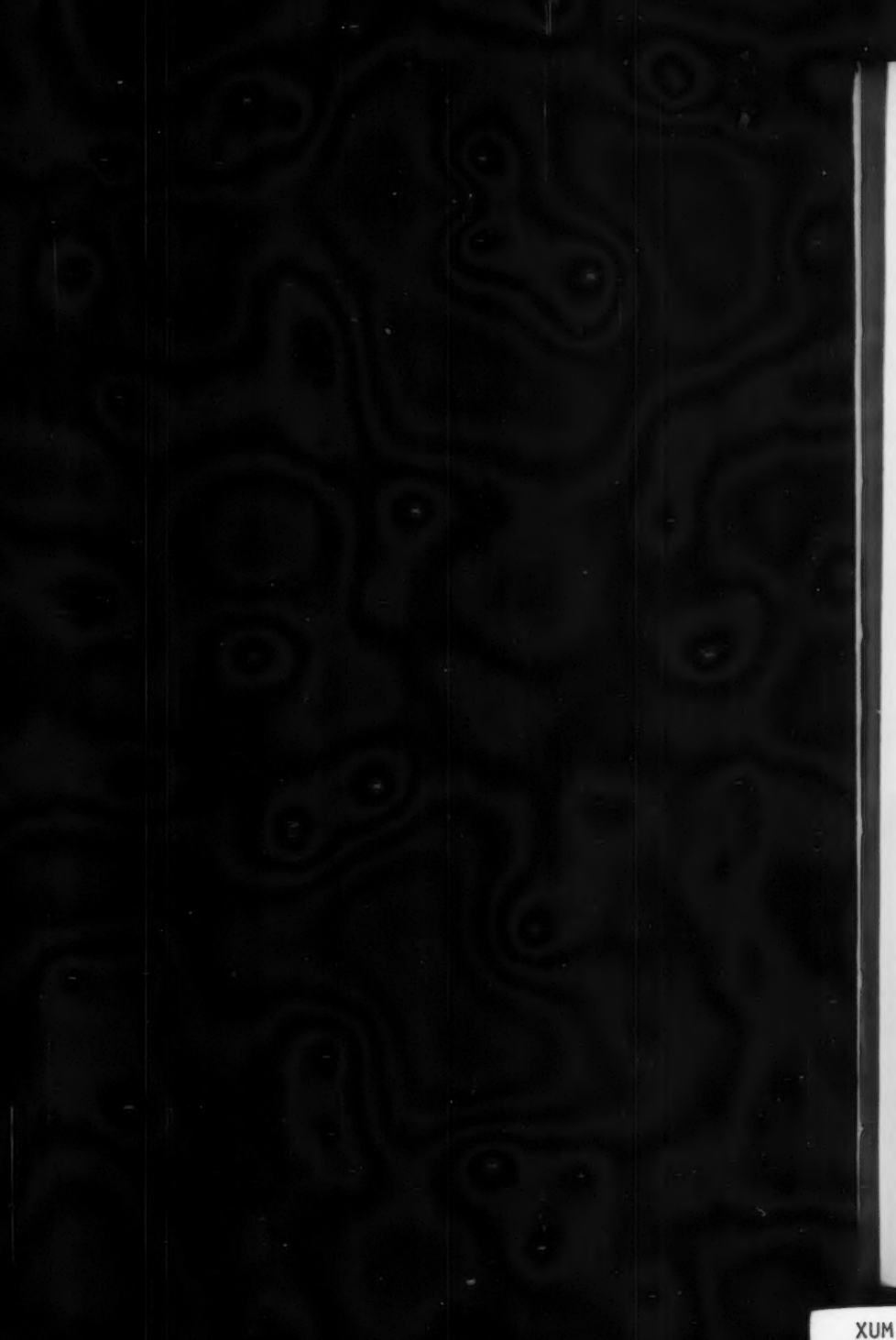

FIG. 1.

FIG. 2.

FIG. 1.—This figure is from an instantaneous photograph taken for me by Dr. E. S. Worrall at the moment of excitation of the second cervical motor nerve root on the left side, and shows the lateral inclination of the head to the side stimulated, so that the left side of the head and face and left shoulder are approximated; the chin is at the same time directed to the right, without being elevated from the thorax to any degree.

FIG. 2.—In this case the instantaneous photograph was taken by Dr. Worrall at the moment of excitation of the fourth cervical motor nerve root on the left side, and shows lateral inclination of the head by which the side of the head and face are approximated on the side stimulated, while the head is drawn back to the left, so that the chin is raised well off the thorax and directed to the right.

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the representation is only slight, so that the resulting movement of the head, while resembling that produced by excitation of the seventh and eighth cervical roots, is very much smaller in amount. In those cases in which the muscle has no representation in the first thoracic root, the resulting movement of the head on excitation of this root resembles that about to be described as characteristic of the second and certain other thoracic roots rather than that produced by excitation of the seventh or eighth cervical roots.

Second Thoracic Root.—The movement characteristic of excitation of this root is a lateral curving of the spine by the action of the erector spinæ, and, as a result of this, the head is slightly tilted laterally towards the opposite side. This is clearly a movement of the head brought about indirectly, and in no way due to any direct action of muscles attached to any part of it. The same movement is met with on excitation of the third and other thoracic nerve roots, even as low in the series as the seventh and eighth sometimes. The movement is always only slight, and, as a rule, it becomes exceedingly insignificant by the time these lower roots are reached.

Muscles represented in each Anterior Spinal Nerve Root.

First Cervical Root.—Rectus posticus major and minor, obliquus superior and inferior, trapezius, sterno-cleido-mastoid, sterno-hyoid, sterno-thyroid, omo-hyoid, sometimes complexus and trachelo-mastoid.

Second Cervical Root.—Complexus, splenius, trachelo-mastoid, cervicalis ascendens, trapezius, rectus capitis anticus major, longus colli (upper part), sterno-cleido-mastoid, sterno-hyoid, sterno-thyroid, omo-hyoid.

Third Cervical Root.—Complexus, splenius, trachelo-mastoid, cervicalis ascendens, trapezius, rectus capitis anticus major, longus colli (upper part), levator claviculæ, sometimes sterno-cleido-mastoid and levator anguli scapulæ.

Fourth Cervical Root.—Splenius, cervicalis ascendens, longus colli (middle part), levator claviculæ, levator anguli scapulæ, sometimes complexus, splenius, and trachelo-mastoid.

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Fifth Cervical Root.—Erector spinæ, longus colli (middle part), levator anguli scapulæ, rhomboidei.

Sixth Cervical Root.—Erector spinæ, longus colli (lower part), latissimus dorsi, sometimes levator anguli scapulæ.

Seventh Cervical Root.—Erector spinæ, longus colli (lower part), latissimus dorsi, scaleni.

Eighth Cervical Root.—Erector spinæ, latissimus dorsi, scalenus anticus, medius and posticus.

First Thoracic Root.—Erector spinæ, scalenus anticus, medius and posticus, sometimes latissimus dorsi.

Second Thoracic Root.—Erector spinæ, scalenus anticus, medius and posticus.

Third Thoracic Root.—Erector spinæ, scalenus medius, sometimes scalenus anticus and posticus.

Fourth Thoracic Root.—Erector spinæ, sometimes scalenus medius.

The Nerve Root Supply of the Neck and Back Muscles.

Rectus posticus major, rectus posticus minor, superior oblique, inferior oblique.—These four muscles almost invariably receive their innervation from the first cervical root alone; but in some instances one or other of them receives an additional nerve supply from the second cervical root.

Complexus.—This muscle derives its most constant nerve supply from the second and third cervical roots, and sometimes from the fourth root in addition to these, while in other animals the first cervical is the additional root to supply this muscle.

Splenius.—Like the complexus, the splenius is innervated most constantly from the second and third cervical nerve roots; but it also receives fibres from the fourth cervical nerve root in some cases.

Trachelo-mastoid.—The nerve roots which most commonly supply this muscle are the second and third cervical, but an additional supply is occasionally derived from the first or the fourth cervical roots, and sometimes the supply is from the third and fourth roots alone.

Cervicalis ascendens.—This muscle is supplied by the

second, third and fourth cervical nerve roots, while lower portions of the erector spinæ, including the transversalis cervicis, are innervated from roots aboral to those supplying this muscle and the trachelo-mastoid.

Trapezius.—Few of the muscles investigated had as constant a root supply as the trapezius, which almost invariably responded on excitation of the first, second and third cervical nerve roots.

Rectus capitis anticus major.—This was the only muscle of the group examined, as it was so difficult to obtain a satisfactory view of the rectus minor and oblique. It responded when the second and third cervical roots were excited.

Longus colli.—The upper portion of the longus colli derives its supply from the second and third cervical nerve roots; the middle portion from the fourth and fifth; and the lower portion from the sixth and seventh cervical roots.

Sterno-mastoid.—The most constant innervation of the sterno-mastoid is from the first and second cervical roots, but it is sometimes also supplied by the third cervical.

Sterno-hyoid, Sterno-thyroid, Omo-hyoid.—These three muscles derive their nerve supply from the first and second cervical roots.

Levator claviculæ.—The nerve supply of this muscle is derived from a slightly higher root level than is that of the levator scapulæ, the third and fourth cervical nerve roots being those excitation of which results in contraction of the levator claviculæ.

Levator anguli scapulæ.—The fourth and fifth cervical nerve roots most commonly send fibres to this muscle; but sometimes the third cervical root does so, and in rarer instances the sixth cervical root is responsible for the part supply of this muscle.

Rhomboidei.—The nerve roots excitation of which resulted in contraction of these muscles were, as a rule, the fifth and sixth cervical.

Latissimus dorsi.—This muscle receives its nerve supply from the sixth, seventh and eighth cervical nerve roots; but in some cases the first thoracic root replaces the sixth cervi-

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cal, so that the two roots which most constantly innervate this muscle are the seventh and eighth cervical.

Scalenus anticus.—Sometimes excitation of the sixth cervical nerve root resulted in contraction of this muscle; it always responded when the seventh and eighth cervical and first and second thoracic roots were stimulated, and sometimes also from the third thoracic root.

Scalenus medius.—This muscle has the most extensive supply of the group, receiving its innervation from the sixth cervical sometimes, the seventh and eighth cervical and the first, second and third thoracic always, and sometimes from the fourth thoracic.

Scalenus posticus.—The scalenus posticus has the same root representation as the scalenus anticus, receiving its nerve supply from the sixth cervical and third thoracic sometimes, and always from the seventh and eighth cervical and first and second thoracic roots.

The Genio-hyoid Muscle.

As it is stated in Quain's "Anatomy"[1] that the genio-hyoid derives its nerve supply from a branch given off by the hypo-glossal, but consisting of fibres derived from the upper cervical nerves, this muscle was carefully observed in three experiments, while each of the upper cervical nerve-roots was in turn excited; but not the slightest evidence of response could be obtained from the muscle.

The Infra-hyoid Muscles.

My results are in complete accord with those obtained by Beevor and Horsley[2] with regard to the nerve root supply of these muscles; and I am further in a position to corroborate the statement made by them that the sterno-hyoid and sterno-thyroid muscles are chiefly represented in the first cervical root, and the omo-hyoid chiefly in the second cervical root; also that the anterior belly of the omo-hyoid is represented chiefly in the first, while the posterior belly of the muscle is represented chiefly in the second cervical root.

[1] Quain, 10th Ed., vol. ii., part ii., p. 302. [2] *Loc. cit.*

The Diaphragm.

During the course of some of the experiments the nerve-root supply of the diaphragm was investigated by the electrical method. The abdominal cavity was opened under such circumstances, and the muscle observed from below, the liver, stomach and other abdominal viscera being drawn gently down so as to allow of a clear view of this wide expanse of muscle. The nerve-roots, excitation of which resulted in contraction of the diaphragm were the fourth, fifth and sixth cervical; the portion of the muscle nearest the middle line on the side of the root stimulated responded from the fourth root chiefly, while the outermost portion of the circumference of the muscle on the same side contracted chiefly when the sixth root was excited; the intermediate portion of the muscle responded chiefly when the fifth cervical root was stimulated. As far as could be observed the portions of the muscle which responded were limited to the side on which the nerve roots were excited; there was no evidence of bilateral action on unilateral stimulation.

Table of Comparison of my Results with the Statements in the last edition of Quain's "Anatomy."

		QUAIN.	AUTHOR'S RESULTS.
Root.		*Muscles.*	*Muscles.*
I. C.	Post.	Inferior oblique Rectus posticus major Rectus posticus minor Superior oblique Complexus	Inferior oblique Rectus posticus major Rectus posticus minor Superior oblique Trapezius
	Ant.	Anterior recti Genio-hyoid Infra-hyoid muscles	——— Sterno-mastoid Infra-hyoid muscles
II. C.	Post.	Complexus Superior oblique (sometimes) Inferior oblique Splenius Trachelo-mastoid	Complexus Trapezius Cervicalis ascendens Splenius Trachelo-mastoid
	Ant.	Rectus anticus major Longus colli Sterno-mastoid Genio-hyoid Infra-hyoid muscles	Rectus anticus major Longus colli Sterno-mastoid ——— Infra-hyoid muscles

		Quain.		Author's Results.	
Root.		*Muscles.*		*Muscles.*	
III. C.	Post.	Complexus		Complexus	
		Transverso-spinales		(Not observed)	
		Splenius		Splenius	
		Erector spinæ		Trachelo-mastoid	
		———		Cervicalis ascendens	
		———		Trapezius	
	Ant.	Rectus anticus major		Rectus anticus major	
		Longus colli		Longus colli	
		Infra-hyoid muscles		———	
		Scalenus medius		———	
		Levator scapulæ		———	
		Sterno-mastoid (sometimes)		———	
		Trapezius		———	
				Levator claviculæ	
IV. C.	Post.	Complexus		———	
		Transverso-spinales		(Not observed)	
		Splenius		Splenius	
		Erector spinæ		Trachelo-mastoid (sometimes)	
				Cervicalis ascendens	
	Ant.	Rectus anticus major		———	
		Longus Colli		Longus colli	
		Scalenus medius		———	
		Scalenus anticus (sometimes)		———	
		Levator scapulæ		Levator scapulæ	
		Trapezius		———	
		———		Levator claviculæ	
V. C.	Post.	Transverso-spinales		(Not observed)	
		Erector spinæ		Erector spinæ	
	Ant.	Longus colli		Longus colli	
		Scaleni		———	
		Levator scapulæ		Levator scapulæ	
		Rhomboidei		Rhomboidei	
VI. C.	Post.	Transverso-spinales		(Not observed)	
		Erector spinæ		Erector spinæ	
	Ant.	Longus colli		Longus colli	
		Scaleni		Scaleni (sometimes)	
		———		Rhomboidei	
		———		Latissimus dorsi	
VII. C.	Post.	Transverso-spinales		(Not observed)	
		Erector spinæ		Erector spinæ	
	Ant.	Longus colli		Longus colli	
		Scalenus medius		Scaleni	
		Latissimus dorsi		Latissimus dorsi	
VIII. C.	Post.	Transverso-spinales		(Not observed)	
		Erector spinæ		Erector spinæ	
	Ant.	Longus colli		———	
		Latissimus dorsi		Latissimus dorsi	
		———		Scaleni	

		QUAIN.	AUTHOR'S RESULTS.
	Root.	*Muscles.*	*Muscles.*
I. T.	Post.	Transverso-spinales Erector spinæ	(Not observed) Erector spinæ
	Ant.	———	Scaleni
II. T.	Post.	Transverso-spinales Erector spinæ	(Not observed) Erector spinæ
	Ant.	———	Scaleni
III. T.	Post.	Transverso-spinales Erector spinæ	(Not observed) Erector spinæ
	Ant.	——— ——— ———	Scalenus anticus (sometimes) ,, posticus ,, ,, medius ,,
IV. T.	Post.	Transverso-spinales Erector spinæ	(Not observed) Erector spinæ
	Ant.	———	Scalenus medius (sometimes)

Muscles.	QUAIN. *Spinal Nerve Supply.*	AUTHOR'S RESULTS. *Spinal Nerve Supply.*
Posterior Division.		
1. Rectus posticus major	1st C	1st C, and sometimes one or other in 2nd C
2. Rectus posticus minor	1st C	
3. Superior oblique	1st, and sometimes 2nd C	
4. Inferior oblique	1st and 2nd C	
5. Complexus	1st, 2nd, 3rd, and 4th C	2nd and 3rd, and sometimes 1st or 4th C
6. Splenius	2nd, 3rd, and 4th C	2nd, 3rd, and 4th C
7. Trachelo-mastoid	2nd C	2nd and 3rd, and sometimes 1st or 4th C
8. Cervicalis Ascendens	(Not separately mentioned)	2nd, 3rd, and 4th C
9. Trapezius	3rd and 4th C	1st, 2nd, and 3rd C
Anterior Division.		
1. Rectus lateralis	1st C	(Not observed)
2. Rectus anticus minor	1st C	(Not observed)
3. Rectus anticus major	1st, 2nd, 3rd, and 4th C	2nd and 3rd C
4. Genio-hyoid	1st and 2nd C	No spinal supply
5. Sterno-mastoid	2nd and 3rd C	1st and 2nd, and sometimes 3rd C
6. Infra-hyoid muscles	1st, 2nd, and 3rd C	1st and 2nd C
7. Longus colli	2nd, 3rd, 4th, 5th, 6th, 7th, and 8th C	2nd, 3rd, 4th, 5th, 6th, and 7th C
8. Levator scapulæ	3rd, 4th, and 5th C	4th and 5th, and sometimes 3rd or 6th C
9. Rhomboidei	5th C	5th and 6th C

Muscles. Posterior Division.	Quain. *Spinal Nerve Supply.*	Author's Results. *Spinal Nerve Supply.*
10. Latissimus dorsi	7th and 8th C	6th, 7th, and 8th C, sometimes 1st T instead of 6th C
11. Scalenus anticus	5th and 6th C, and sometimes 4th C.	6th, 7th, and 8th C, and 1st and 2nd T, and sometimes 3rd T
12. Scalenus medius	3rd, 4th, 5th, 6th, and 7th C	6th, 7th, and 8th C, and 1st, 2nd, 3rd, and sometimes 4th T
13. Scalenus posticus	5th and 6th C	6th, 7th, and 8th C, and 1st and 2nd T, and sometimes 3rd T

Comparison of the Tabular Statements.

On comparing these tabular statements of my results with similar statements taken from the last edition of Quain's "Anatomy" we find the following discrepancies, which I propose to point out root by root. No mention is made of the trapezius being supplied by the first cervical root, a supply which I constantly observed; while the complexus is said by Quain to be represented in this root, but I only found it exceptionally represented. Owing to the difficulty of obtaining a satisfactory view of the muscle, I did not investigate the rectus anticus minor, so that it may be represented in this root as stated by Quain; but with regard to the rectus capitis anticus major, I could observe no response in the muscle when the first cervical root was excited. The genio-hyoid is said by Quain to be supplied from this root; but in the three experiments in which I paid special attention to the innervation of this muscle, I could obtain no response on excitation of the first cervical root.

As in the first root, so in the second, Quain makes no mention of the trapezius being represented, while I found it constantly so. Then again, no mention is made of the cervicalis ascendens, which I find supplied by this root. The inferior oblique is included by Quain with those muscles constantly supplied by this root, while in my experience such a supply is exceptional. With regard to the genio-hyoid, what was said when speaking of the first cervical root

applies here also; no response was obtained in the muscle on excitation of this root.

Like the first and second roots, the third, I find, innervates the trapezius, but no mention of such an innervation of this muscle is made by Quain. No special mention is made of the trachelo-mastoid and cervicalis ascendens, both of which I find represented in this root; but it is stated broadly in Quain that the "erector spinæ" is innervated from this root. The infra-hyoid muscles are given by Quain as represented in this root; but, like Beevor and Horsley, I only find these muscles represented in the first and second cervical roots.

The supply of the scalenus medius and levator scapulæ, and the occasional supply of the sterno-mastoid from this root, as stated in Quain, I have been unable to corroborate. It is interesting to note in this connection that, while I failed to find the levator scapulæ represented in this root, I nevertheless found the levator claviculæ (a muscle well developed in the monkey) so represented.

In the fourth root the complexus is included, in Quain's description, among those muscles constantly represented, but I find such a representation exceptional, and in no case have I found the complexus represented in the first and fourth cervical roots together, as stated in Quain. I do not find the rectus capitis anticus major represented in this root, while Quain does. The same statement applies to the trapezius, and scalenus anticus and medius. Then, again, I find the trachelo-mastoid sometimes supplied from this root, while no mention of such a supply is made in Quain.

With regard to the fifth cervical root, our only point of difference is as regards the supply of the scaleni. That they are innervated from this root, as stated by Quain, I have not been able to find any evidence.

In the sixth root I only find the scaleni sometimes represented, and not constantly, as stated by Quain. On the other hand, I find the latissimus dorsi frequently represented in this root, while no mention of its being so represented is made by Quain.

The seventh cervical root I find supplying all the scaleni;

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but Quain only mentions the scalenus medius as supplied from this root. I find the scaleni supplied by the eighth cervical root, but no such supply is mentioned in Quain, whereas the longus colli is said to be innervated by this root, contrary to any experimental evidence I have been able to obtain on this point.

In that I further find the scaleni supplied by the upper three thoracic nerves, and the medius also from the fourth sometimes, while Quain gives the seventh cervical as the lowest root in which any of these muscles are represented; the discrepancy is very great.

General Remarks.

The impression with which I am left after comparing the results of this investigation with those I obtained formerly in connection with investigations into the functions of the spinal nerve roots responsible for the nerve supply of the muscles of the limbs, is that there is considerably more individual variation with regard to the nerve root supply of the neck muscles than of those of the limbs.

As in the case of the limb muscles, so in those of the neck, it was abundantly evident that different segments of any given muscle responded when one or other of the nerve roots supplying the muscle was excited electrically. In some muscles this differentiation of parts was such that, instead of the whole longitudinal extent of the muscle contracting on stimulation of a given root, where the muscle derived its nerve supply from three roots the upper portion would alone respond on excitation of the highest root of the series from which it derives its supply, while the middle portion would alone do so when the intermediate root of the series was stimulated, and the lower portion when the lowest of the three roots was excited. In other muscles, instead of its being a segment above or below another segment of a muscle which responds on excitation of this or that nerve root, the whole length of muscle responds, but only in so far as one part of it is concerned, so that if supplied by two nerve roots, the whole longitudinal extent of one lateral half of the muscle

would respond, while the other lateral half of it would remain quiescent in so far as active contraction of the muscle fibres are concerned, any movement in this part of the muscle being only brought about indirectly by being pulled on by the actively contracting portion which is contiguous to it.

This differentiation of one part of a muscle from another by means of its nerve root supply served as a means of differentiating different parts of a complex muscle such as the erector spinæ in a most interesting manner. Thus excitation of the second, third and fourth cervical nerve roots resulted in contraction of different lateral parts of the cervicalis ascendens muscle, the inner portion of the muscle, *i.e.*, nearest the middle line dorsally, responding chiefly from the second root, the outer portion of the muscle, *i.e.*, that farthest away from the middle line posteriorly, responding chiefly from the fourth root, while the intermediate portion of the muscle responded chiefly from the third root. On excitation of the fifth cervical nerve root the portion of the erector spinæ which now responded was that which is nearest to the middle line dorsally, altogether below the lower limit of the cervicalis ascendens, while excitation of the sixth root resulted in contraction of a portion of the muscle a little farther from the middle line dorsally, and so on.

On comparing the combined movement which results on excitation of a given nerve root with the individual muscles actually represented in the same root, it becomes obvious that the resulting combined movement must depend on the preponderance of certain muscles over others, which, if acting alone, would bring about a very different movement.

The muscles deriving their nerve supply from any given nerve root occupy both the dorsal and ventral aspects of the neck and trunk. Despite the variations met with as regards the precise muscles represented in a given root, or the roots supplying a given muscle, the movement resulting on excitation of each root remains wonderfully constant, such variations as are met with being so slight as not to alter materially the predominating movement characteristic of each root.

In the case of such small short muscles as the posterior recti and obliques, the nerve supply appeared to be derived

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from a single nerve root in most cases, but this is quite exceptional, as all the other muscles of the neck and back investigated derived their nerve supply from two, three, or more nerve roots.

Practical Bearing of the Results on the Treatment of Wry-neck.

A knowledge of the precise muscles innervated by any given nerve root cannot fail to be of great advantage in any consideration as to the nerve roots, division of which is most likely to be attended with relief of the spasm in wry-neck. This is, of course, of special value with regard to those muscles sufficiently superficial in position to allow of their being seen or felt to be in a state of spasm. But the knowledge is also of use to us with regard to muscles too deeply seated to be seen or felt with certainty, for a knowledge of the movement brought about by the action of this or that muscle enables us to ascertain fairly satisfactorily which muscles are probably in a state of spasm.

Important as is this knowledge of the precise innervation of the various muscles responsible for the movements of the head on the trunk, no less important is the information which we derive from those experimental results in which the position assumed by the head in relation to the trunk was noted. The results obtained in this connection point clearly to the fact that in the most common forms of torticollis surgical treatment must be directed, chiefly at all events, to one or all of the four upper cervical nerve roots. It is further evident that where the lateral inclination of the head, by which the shoulder and side of the head and face are approximated, exists, the first and second cervical nerve roots are those to which attention should be directed, while in those instances in which the chief movement of the head is one in which the occiput is drawn backwards, so that the face looks more or less upwards, the third and fourth cervical nerve roots should engage our attention.

The results further point to the fifth and sixth cervical nerve roots being the least likely, of the cervical series, to

conduct impulses from the central nervous system, the cutting off of which are likely to be followed by any material relief in any muscular spasm which may be present causing wry-neck.

It is also important, with a view to prognosis after such operations, that even muscles supplied by the seventh and eighth cervical nerve roots may by their spasm bring about movements of the head on the trunk indirectly, in which connection the trivial indirect movement of the head resulting on excitation of the upper thoracic nerve roots may not be altogether without a certain amount of importance.

One class of cases of wry-neck, happily not common, is placed beyond the possibility of surgical relief, as far as division of nerve roots is concerned, by a consideration of the results obtained in this investigation. I allude to those cases in which the spasm is limited to, or chiefly involves, the scaleni muscles, which, as we have seen, are innervated from the sixth or seventh cervical to the third or fourth thoracic nerve roots. Apart from the obstacle to interference with the lower cervical and first thoracic nerve roots, owing to the innervation of the upper limb being derived from these roots, that accelerator and augmentor fibres of the heart are contained in the upper thoracic roots[1] may be a point not to be altogether neglected in any operation directed to these roots.

A not unimportant consideration in prognosis after such operations for wry-neck is the question whether in those cases, for instance, in which a muscle is represented mainly in two nerve roots, but also to a lesser degree in a third, the full force of nervous discharge may reach the muscle by this comparatively insignificant path now that it can no longer reach the muscle by the main paths. That this is not probable is suggested by the fact that any given nerve root only supplies a portion of muscle and not the whole muscle, as I have shown in this and previous papers; and that, in a case such as that supposed above, the portion of muscle would be very small, and, therefore, even if receiving the

[1] cf. Bradford and Dean, *Journal of Physiology*, vol. xvi., 1894, p. 34.

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full force of the discharge from the central nervous system, the muscular fibres capable of responding are too few to give anything like an adequate exhibition of the full force of the central disturbance. As further supporting this view, may be instanced the fact that, in investigations in which the spinal nerve roots which contribute to the formation of the limb plexuses were the subjects of experimentation,[1] I showed that division of one or more nerve roots altered the position assumed by the limb during general convulsions subsequently induced, by the intra-venous injection of absinthe, in a manner directly proportional to the amount of this or that muscle excluded from taking part in the convulsions, in consequence of the division of one or more of the nerve roots from which it is innervated.

But as opposed to these considerations must be borne in mind the fact that, in the investigations dealing with the limb plexuses, I further showed that the power of recovery after division of a nerve root was wonderful, and that this occurred when all possibility of reunion of the divided ends of the roots taking place was placed beyond question. The immediate effect of the division showed itself in the altered position of the limb in standing, or of its movements in progression, weakness of one or more movements of the limb being evident; but in a wonderfully short time all traces of these defects passed off, and it became difficult to detect any abnormality in the position of the limb in standing, or of its movements in progression. That the provisions for compensation were great was evident, but it was not so easy to explain how this compensation was brought about. Of the theories that might be advanced in explanation of the fact, two appeared to me to be worthy of consideration. The first of these supposed it possible that certain cells of the cerebral cortex were responsible for impulses which could reach the same muscle by different nerve roots, and that the division of one of these nerve roots by which such impulses pass might produce a reflex inhibitory effect[2] on the cortical cells from which the impulse it conducts are

[1] *Phil. Trans. Roy. Soc.*, vol. clxxxiv., 1893, p. 41; and *Proc. Roy. Soc.*, vol. liv., 1893, p. 243.

[2] cf. Bubnoff and Heidenhain, *Pflüger's Archiv.*

derived, so that the amount of actual loss of motor power in the first instance is more than that resulting from the exclusion of the parts of the muscles innervated by the divided root, and that as the cerebral cells recover from the inhibitory action so power is restored to the parts of the muscles still in connection with the central nervous system. The second hypothesis supposed no such inhibitory effect on the cells of the cerebral cortex; these cells were regarded as being capable of discharging the same amount of energy as before, but the impulses meet with a block, owing to the division of the fibres along which they formerly passed. Gradually, however, the impulses are diverted, it may be through the anterior horn cells of the spinal cord along other channels, and thus, in time, the fibres of the muscle which are supplied by an intact root receive almost the same amount of stimulation as did the whole muscle formerly.

Between these two hypotheses there seemed little to choose, when dealing with the limb muscles, most of which received their innervation from two roots, and the difference of the amount supplied by the two roots was not too great to make either of these theories untenable.

A third possibility suggests itself to me as worthy of passing consideration. Is it possible that the normal stimulus reaching the fibres of the part of a muscle still in connection with the central nervous system is capable of inducing secondarily a contraction of other fibres of the same muscle no longer directly connected with the central nervous apparatus? In this connection it is worth remembering that, even after complete section of all the nerve fibres to a muscle, the muscle fibres do not become fattily disintegrated, but undergo a very slow process of atrophy. It is not unreasonable, therefore, to suppose that when only part of the nerve fibres of the muscle are divided the muscle fibres formerly in direct connection with them may be prevented from undergoing any great amount of this simple atrophy even, being brought into physiological action indirectly through the agency of some influence derived from the muscle fibres which are still in connection with the central nervous system.

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NOTES OF TWO CASES OF PERIPHERAL NEURITIS, WITH COMPARATIVE RESULTS OF EXPERIMENTAL NERVE DEGENERATION AND CHANGES IN NERVE CELLS.

BY ROBERT A. FLEMING, M.D., F.R.C.P.E.

In this paper I wish to give an account of several cases of peripheral neuritis, mainly from the pathological point of view, to give a brief *résumé* of the results of my experimental nerve sections in dogs and rabbits, with a note of the changes found in nerves of stumps at varying dates after amputation, the changes in nerve cells of ganglia and cord on the affected side, and thereafter to show the bearing of certain of the results on peripheral neuritis.

So much has been written about peripheral neuritis—so many views formulated only to be afterwards demolished—that a disease of such varied modes of origin and clinical history requires to be attacked from many different standpoints. Perhaps there is no better method of approaching the problem than from the experimental side, and, although rabbits' nerves differ from those in man, the results obtained appear to me to throw an instructive light on the pathological anatomy of peripheral neuritis.

By the term "peripheral neuritis" I understand an affection primarily of the neuron, in whole or in part, and not a disease having its origin in some other part of the spinal cord.

The cases referred to will be published in full at a later date; all I wish to do here is to bring out the prominent

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features, clinical and pathological, for the purposes of this paper. I wish to draw special attention to the changes in the vasa nervorum and the minute fibres in the affected nerves and in the nerve cells of the cord in one of the cases. No reference is made to the bibliography of the subjects discussed. I did not have the opportunity of examining Case I. during life.

Case I.[1]—Mrs. H., aged 38, admitted to Ward 25, Royal Infirmary, Edinburgh, on April 2, 1894, complaining of debility, great weakness of the hands, with almost complete loss of power in the feet and legs, with numbness and a sensation of "pins and needles" in the legs and arms. The weakness in the legs had lasted a fortnight; the numbness and loss of power in the lower limbs had been present for four days. She had six healthy children, all living—no miscarriages, and nothing to report in the family or personal history of special interest. Her last labour occurred three months before the commencement of the illness for which she was admitted, but it appears to have been normal in every respect.

The illness began a fortnight beforehand with a shivering attack, and she was confined to bed for one day. She resumed her household duties with difficulty for one week, and then was compelled to take to bed again. She had now severe pain in the back, relieved on sitting up. She tried to get up several times, but was unable to do so.

She had not been well fed, and was given to drink, locking herself into her room for the apparent purpose of indulging in that vice, and she admitted that during New Year week they had consumed nine bottles of whisky without help from visitors. There was no history of diphtheria or sore throat.

On admission—

Sensory Functions.—Pain in small of back, increased by palpation; numbness in hands and feet, and "pins and needles" sensation in hands.

Tactile Sensibility.—Unimpaired localisation and rate of conduction perfect.

Pain Sensation.—Apparently perfect.

Temperature Sense.—Impaired; delayed conduction, but she could distinguish heat from cold.

[1] History of case abstracted from Edinburgh Hospital Reports, vol. iii., p. 417.

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Case I.

CERVICAL ENLARGEMENT.

Group.	RIGHT ANTERIOR CORNU.				LEFT ANTERIOR CORNU.			
	Cell.	Nucleus.	Nucleolus.	Endo-Neucleolus.	Cell.	Nucleus.	Nucleolus.	Endo-Neucleolus.
Ventro-mesial	56μ × 40μ	22μ × 20μ	8μ	2·5μ	84μ × 60μ	26μ × 26μ	8μ	3μ
	44μ × 24μ	14μ × 12μ	8μ	3μ	80μ × 44μ	22μ × 22μ	6μ	3μ
Ventro-lateral	80μ × 34μ	20μ × 16μ	10μ	3μ	76μ × 50μ	26μ × 20μ	8μ	3·5μ
	30μ × 24μ	14μ × 12μ	8μ	2·5μ	68μ × 60μ	24μ × 24μ	10μ	4μ
Dorso-lateral	64μ × 40μ	20μ × 14μ	10μ	4μ	60μ × 50μ	9μ × 9μ	6μ	3μ
	44μ × 30μ	14μ × 12μ	8μ	3μ	50μ × 40μ	16μ × 14μ	8μ	4μ

NOTE.—The measurements of the cells and nuclei represent their greatest length and breadth, and in the case of the cells are taken from the roots of the processes.

Muscular Sense.—Noted as markedly impaired.

Skin Reflexes.—Plantar on each side abolished; others present.

Tendon Reflexes.—Patellar reflex absent on both sides.

Voluntary Movements.—Distinct pointing of the toes on both sides, due to a condition of talipes equinus since birth. Patient could not draw up her knees, but, if flexed, she could extend them. The inability to use her flexors was most marked on the right side. Grasp was feeble, and the flexors and extensors of the forearm were weak. The muscles of the upper arm were a little stronger.

Muscles of lower extremity did not react to faradism; but reaction was obtained in the upper extremities, though weak, but was specially deficient on the right side.

Vasomotor symptoms were absent, except a slight cyanosis of the feet and distension of the superficial veins.

Patient became extremely weak and irritable; complained of great pain in the back, which precluded a thorough investigation of the case.

Patient became cyanosed on April 5, at 9.15 a.m., from paralysis of the diaphragm and lower intercostal muscles, and artificial respiration, by Silvester's method, was kept up continuously for sixty-eight hours, when she died from exhaustion. Ether and strychnine were administered subcutaneously. She could swallow up to a late stage. Muscular weakness rapidly increased, the right arm being much weaker than the left.

Micturition and defæcation remained unaffected up to a late stage.

Post-mortem examination performed by Dr. Leith yielded nothing worthy of note.

Microscopical Examination.

The pathological examination of the nerves, etc., I made in the Practice of Physic Laboratory, Edinburgh University. The brain, pons, medulla, cord, and the following nerves were examined:—The brachial plexuses; the right ulnar; the sciatic, posterior tibial, and the right anterior crural nerves; the nerve to the left gastrocnemius muscle, with a portion of the muscle attached; the fourth right intercostal nerve; the phrenic nerves, with portions of the diaphragm; the vagi nerves and the sympathetic on the right side, with the superior cervical ganglion. Unfortunately, the external popliteal nerves were not obtained, nor any of the ganglia on the posterior nerve roots.

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The changes met with were more remarkable than any I have seen described in the literature of peripheral neuritis, and the case was a unique one, inasmuch as the disease only lasted about twenty days from the commencement of symptoms, and the function of the intercostals was only seriously affected from seventy to eighty hours. On examining the brain I thought it might be worth while to try a modification of Golgi's method, described by Berkeley,[1] for the demonstration of dendrites, in a paper on lesions produced by the action of ethylic alcohol on the cortical nerve cells. Even though my specimens were somewhat over-hardened for this purpose in Müller's fluid, I found, in thus treating a portion of the left cortical motor area for the arm, that several of the ganglion cells showed the same marked bulging on their dendrites which has been described by Berkeley. The terminal dendritic swelling is the most marked, though, on close examination, other small moniliform swellings can be seen affecting several dendrites. None of the swellings appeared quite close to the cell body. These dendritic changes are best marked in the cortex of the motor areas, but were also noted over a wider area, and were seen in the cell groups forming the ganglia of the cranial nerves. The gemmulæ of the dendrites did not come out well, but they appear to be diminished in number, at least on the affected processes.

In the Cord.—The changes here are of the greatest interest. There are a great number of recent and older hæmorrhages best marked in the cervical and dorsal regions, and mostly found in the grey matter. At the level of the cervical enlargement a small encapsulated hæmorrhage is seen in the right antero-lateral ground tract, and is evidently from a centrifugal arteriole. The extravasation is surrounded by a laminated, hyaline wall, and there are a small number of leucocytes in the neighbourhood. In the course of the same arteriole, and nearer the anterior cornu, are several other smaller and apparently more recent hæmorrhages.

In the dorsal region, at the level of the second dorsal segment, one of the main branches of the anterior spinal artery to the right side of the canal is cut transversely and is seen to be blocked with a recent thrombus surrounded by numerous leucocytes. At the level of the ninth dorsal segment a large recent hæmorrhage is seen along the line of a branch of the anterior spinal artery, running to the outer side of the posterior cornu, in one of the septa. Here there is no great emigration of leucocytes.

[1] BRAIN, Part VI., 1895.

These hæmorrhages are limited to the anterior spinal artery and to the right side of the cord. Beyond the hæmorrhages of recent date, there are no vascular changes such as are seen in the nerves. The tracts of white matter show practically nothing. The nerve cells in the cord, although fixed in Müller's fluid, and, therefore, not so capable of showing nuclear changes, still demonstrate certain interesting facts, because the multipolar cells on the two sides of the cord show distinct differences. On the left side the cells are practically the nerve cells of a normal specimen hardened in Müller's fluid. On the *right* side we find great shrinking of the cells as a whole (see table of measurements). The nuclei are much smaller, and, in place of having a distinctly rounded contour, their outline is jagged and totally irregular. On the *left* side the cells show a clear, distinct nucleolus and endo-nucleolus, and a rich refractile chromatin network (see fig. 1). On the *right* side the whole nucleus stains more deeply and homogeneously, the nucleolus is with difficulty made out, and it is sometimes impossible to distinguish the endo-nucleolus. There are one or two healthy cells in each cell group of the *right* anterior horn ; and there are rather larger numbers of degenerated cells in the groups of the *left* anterior horn ; still the degenerated cells on the one side and the apparently normal cells on the other are quite characteristic. Anilin blue black reveals further that the affected cells possess fewer processes.

The ventro-mesial group of nerve cells is believed by Kaiser and others to be the centre for the nerves of the dorsal muscles, and the ventro-lateral group for the lateral and ventral muscles of the trunk and the muscles of respiration. In the cervical region the spinal accessory nerve is supposed to arise from the lateral group, the phrenic from the mesial. Now in this cord the ventro-lateral cell group is very markedly affected, and the inference is obvious. In this case, the patient had paralysis of the diaphragm on the right side more markedly than on the left, and the right intercostal muscles appeared also more markedly affected than the left. The greater number of degenerated cells at this level on the left side are in the ventro-lateral group, although there are a number of apparently healthy multipolar cells in addition.

As I chanced to have another cord with hæmorrhages at more than one level, and especially in the region of the cervical enlargement, I took several sections and made measurements of the cells and nuclei, selecting the largest cells from the different groups, and I noted that the mere effect of hæmorrhages being present in proximity to the cell groups made no difference in the

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respective size of cells and nuclei, although the hæmorrhages in this case were very extensive.

The nerves were examined with particular care, and not merely were a few sections made from each, but many series of sections were examined, and portions of the same nerve were investigated at different levels.

In not a few of these nerves, the same description will suffice. In the *right sciatic, right anterior crural, left sciatic*, etc.

The Weigert-Pal specimens show many changes with only about half the fibres stained, and many that stain do so in a manner suggestive of an early stage of degeneration. Whereas this method stains most deeply the peripheral portion of the medullary sheath of a normal fibre, leaving a clear and faintly-stained central part, sometimes with a darker ring closely surrounding the axis-cylinder itself, a fibre in an *early* stage of degeneration stains often uniformly thoughout and uniformly deeply. In fact, with a carefully-stained section, if properly decolourised, a uniformly black fibre, with sometimes a clear central spot representing the axis-cylinder, would point to the degeneration of the myelin alone, or what Gombault describes as the condition constituting periaxial neuritis, whereas disappearance of the clear spot in the centre implies that the continuity of the axis-cylinder is broken as well as the myelin.

The most degenerated fibres are in groups arranged throughout the funiculi. The fibres staining a uniform colour are close to these groups, or are intimately mixed up with the fibres composing them. The healthy fibres are also generally arranged in groups together. Specimens stained with hæmatoxylin and eosin show a considerable number of comparatively healthy fibres in most of the funiculi, and the grouping of the degenerated fibres is very marked. Careful examination of the various funiculi shows that there are very few fine fibres, and that those which are degenerated appear to take their place. These fine fibres occur in groups, and I shall have to refer to them again at length.

In connection with the degenerated groups and the funiculi generally, there are a number of nucleated cells. Some of these nuclei are very large in size.

(*a*) Many belong to the spindle-shaped cells, whose protoplasm is very small in amount, staining brightly with eosin and showing marked signs of mitosis; these are newly-formed, connective-tissue cells.

(*b*) Many are leucocytes, and these are chiefly in the neighbourhood of vessels, although they have spread amongst the

surrounding nerve fibres, being specially numerous near the little degenerated groups mentioned already. (See fig. 2.)

(*c*) A smaller proportion are enlarged and proliferated segmental nuclei, but, unless in fibres which have broken down, these can be seen inside the sheath of Schwann, and projecting into the lumen of the nerve fibre.

The axis-cylinders do not stain so sharply, and may be seen, even in transverse section, to be finely granular. There are few signs of proliferation of the segmental nuclei, though they are generally much distended, bulging into the lumen of the fibre. The larger septa of the endoneurium are not thickened; the finer septa, especially those separating individual fibres or smaller groups, show greatly enlarged and evidently proliferated connective tissue nuclei, but only in the neighbourhood of degenerated fibres, and these are also very marked in the proximity of vessels, and near the inner surface of the perineurium. The perineurium is very slightly thickened, but only in parts. The thickening is limited to the inner layers, and where vessels of small size are met with in the sheath, the newly-developed connective tissue nuclei are very numerous.

The epineurium shows little that is abnormal. The vessels are specially interesting because they illustrate the changes described by Minskowski in 1888, and later by a number of writers. Almost all the smaller arterioles and capillaries in the endoneurium or perineurium of those funiculi showing many degenerated fibres demonstrate the changes about to be described, though comparatively few in the epineurium do so. These changes are—distension and proliferation of the nuclei belonging to the endothelial cells of the intima, and to a less extent a similar nuclear increase in the media and adventitia. Comparatively few capillaries are so nearly blocked by the huge endothelial nuclei as in the more peripheral nerves. A considerable number of these nuclei show karyokinetic figures. The walls of the smaller arteries and arterioles are distinctly thickened. Almost every vessel within the perineurium seems to be surrounded by a greater or smaller number of leucocytes, and a huge diapedesis is shown in fig. 2, taken from one funiculus of the nerve. There is evidence of a considerable amount of exudation, which is greatest between the perineurium and the nerve fibres, and also in the neighbourhood of the arterioles and capillaries, well seen in fig 3.

In the more peripheral nerves the arterioles and capillaries have suffered even more markedly, whether in endo-, peri-, or

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epineurium; they all show marked proliferation of nuclei, especially those in the intima, being greatly enlarged and showing very distinct mitotic figures. Many capillaries are so occluded that a red blood corpuscle could scarcely, if at all, pass through their lumen. The venules also show proliferation of endothelial nuclei. Corpora amylacea are present in many of the nerves more markedly degenerated.

The nerve to the left gastrocnemius muscle shows beautifully the effects of a recent large exudation, best marked in the neighbourhood of the vessels. It is present not merely between the nerve fibres and the perineurium, but it is well seen also along the septa. The specimen has been extremely well fixed, and the paraffin sections give what I believe to be a truthful picture. The effects of the exudation are very apparent; those fibres which have been pressed upon show marked degenerative changes, namely, granular myelin and irregularly distended and granular axis-cylinders. The fibres of the endoneurial septa and the inner lamellæ of the perineurium are separated by the effusion, and present an open network-like appearance. The exudation itself forms fibrinous threads or strands, here and there replaced by newly-formed connective tissue cells, and a considerable number of leucocytes are in its vicinity. The nerve fibres at some distance from the exudation are mostly normal, but still some show granular changes in the myelin and distended irregular axis-cylinders, staining feebly with eosin or benzo-purpurin. On the other hand, certain larger fibres appear to resist pressure more than others, but the comparative rarity of these, where the exudation is great in amount, is very striking. The fine fibres have evidently suffered most severely, and the connective tissue nuclei, increased in size and number all over the funiculi, are specially prominent near groups of what had once been these fibres. The segmental nuclei are enlarged, but there is little evidence as yet of proliferation.

The vessels of the endo- and perineurium show marked nuclear increase in the intima. The media of the arterioles in the same localities is thickened.

All the funiculi have not suffered alike, but all show some exudation, appearing first around the vessels in the neighbourhood of the perineurium.

Some of the nerves—as, for instance, the right ulnar, and especially the intercostals—show much more advanced changes in the myelin and axis-cylinders; and in these nerves, corpora amylacea are extremely numerous.

In several of the nerves of this case I endeavoured to study:—

(1) Whether the leucocyte emigration was confined to parts of the nerve or was generally met with throughout its length?

(2) Whether the diapedesis of red blood corpuscles was local?

(3) Whether the degenerated fibres (which could be fairly well followed in groups by noting their relationship to the blood-vessels) were healthy at any part of their course; or, if not, whether the degeneration varied at different levels as stated by many writers?

(4) Whether the nuclear changes in the blood vessels varied at different levels, and if they had any relationship to the phenomena noted under the other three headings?

The *phrenic nerves* were selected because they were small, and there was some chance of recognising the approximate position of fibres, or even identical fibres, in each section. My results are, in brief:—

(1) The leucocyte emigration varied greatly in different portions of the nerve, and where one vessel showed marked evidence of emigration most of those in the funiculus did the same. The leucocyte emigration tended to increase in the phrenics from above downwards—being most marked close to the diaphragm.

(2) The diapedesis of red blood corpuscles was equally local, and the emigration of leucocytes and diapedesis of red blood corpuscles in the nerves generally occurred together; but diapedesis of red blood corpuscles was less frequent.

(3) The degenerated fibres in the phrenics showed the most interesting changes. The fine fibres very gradually appeared to decrease in number from above downwards to the diaphragm, probably leaving the nerve to supply minute vessels *en route*. The larger fibres seemed to suffer more from the effects of pressure at a higher level than from a true parenchymatous degeneration. They were more dropsical-looking, *i.e.*, more distended, and of less regularly round and oval outline below as compared to the most

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proximal sections. The medullary sheaths were granular in appearance, but the axis-cylinders were generally perfectly recognisable. One or two groups of markedly degenerated fibres in the right phrenic I followed carefully. These at one level were compressed together, by a large exudation and many leucocytes, so as to form a mass of myelin with unrecognisable sheaths of Schwann, with segmental and connective tissue nuclei, a few leucocytes, and even one or two axis-cylinders apparently unaffected. At a lower level these fibres had not recovered from their compression, and, although they gradually separated to some extent, they remained distended, and were not, in this nerve at least, absolutely normal-looking below. Apparently, with the possible exception of a few fibres, the changes in this nerve were not due entirely to a parenchymatous neuritis, but were largely the result of pressure, causing a secondary descending degeneration.

(4) The nuclear changes in the vessels vary very considerably at different levels, being much better marked where the emigration of leucocytes and red blood corpuscles were most in evidence, and less marked between.

Case II.—Mrs. M., aged 38, was admitted to Ward 25, Royal Infirmary, Edinburgh, on November 4, 1895, suffering from paralysis of the arms and legs, difficulty in swallowing, and loss of the power of speech.

She had a healthy family history, has had ten children, eldest aged 19; youngest, 3 years. Three are dead—one of bronchitis, and two of chest disease; the others are strong and healthy.

Patient has had no other illness, but had suffered from an occasional sore throat, yielding generally to local applications. Her mother suffered in the same way, and, in both cases, any slight exposure to cold was sufficient to bring on an attack. Mrs. M. had been very alcoholic, but to what extent could not be ascertained; though it is admitted that, during the greater part of her married life, she had been imbibing larger and larger quantities of spirits, beer, &c. She was rarely actually drunk, but often slightly stupefied.

There was no diphtheria in the district at the time of patient's illness, nor had the patient anything suggestive of the disease. She simply complained of one of her usual sore throats, which she

attributed to exposure to cold, and which on this occasion rapidly yielded to treatment. The patient never had any serious illness since childhood. There was no history of influenza as an antecedent to her condition, nor had she previously been attacked by it.

Three months ago the patient complained, for the first time, of numbness of the legs and swelling of the feet. These gradually increased, and patient was known to have resorted to an extra allowance of alcohol in the hope of curing the unpleasant symptoms. Patient took to bed on October 21, 1895, having only previously been laid up when the numbness and swelling incommoded her, as it did to an increasing extent; and she found a day's rest appeared to give some relief. She now had shooting pains and twitchings in her left arm, and, to a less extent, in her legs. On October 29 she found great difficulty in swallowing, and the power of speech was gradually lost. The patient, in a few days, became semi-comatose.

Notes on Admission.—She is a stout, flabby woman, with a somewhat bloated expression of countenance. There is nothing specially worthy of note in any system, excepting the nervous. The heart and lungs show nothing of special interest; the temperature, which was normal when the patient was admitted, rose to 102° Fahrenheit, where it remained for two days before death, and the pulse became at the same time correspondingly rapid.

Sensory Functions.—As the patient is almost unconscious, can be only with difficulty roused to the slightest extent, and is totally unable to speak, her subjective symptoms are unobtainable. Pain is evinced on pressure over the nerve trunks, and over certain muscles. The pupils are equal, and react sluggishly to light. The fundus of both eyes is normal.

Motor Functions.—Swallowing being almost impossible, the patient is fed by the rectum. Breathing is slow, somewhat stertorous, and from time to time simulates the Cheyne-Stokes type. The bowels and bladder are evacuated involuntarily. Skin reflexes and tendon reflexes of the legs and arms are abolished.

Electrical Reactions.—In examining the electrical reactions, I had the assistance of Dr. Garbut. On the first occasion the patient's skin was extremely hyperæsthetic, and it was very difficult to get accurate results, but our later efforts were more successful. One of the outstanding features was that a very powerful faradic current had to be used, far in excess of what a normal individual could endure. We tried at first a current which was too weak, although producing considerable pain when applied to our own persons, and with this we only got, almost universally,

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a negative result. We next tried as powerful a current as we could obtain from a large induction coil; and when the skin was sufficiently anæsthetic to permit of observation, we noted that with nearly every muscle which had given us negative results before we now got a contraction, though, in some cases, an extremely feeble one; and this contraction was sometimes coincident with the qualitative change in the reaction of degeneration. Where the qualitative change, constituting one of the phenomena of the reaction of degeneration, was observed, the contraction was invariably noticed to be very sluggish, and obtainable with a smaller number of cells than usual.

The muscles of the extremities were examined in detail. It seems hardly necessary here to give the tables I prepared. Suffice it to say that the peronei and the tibialis anticus muscles of the left leg especially and some of the extensors and flexors of the fingers and the small thumb muscles of both arms gave the reaction of degeneration.

Vaso-Motor and Nutritive Functions.—There is slight œdema of both ankles. There is no evidence of any bedsore. None of the muscular groups are very markedly wasted—certainly the tibialis anticus and the calf muscles show considerable atrophy. The right leg is slightly smaller in circumference than the left. Both buttocks are flattened. The muscles of both fore-arms are flabby. Sweating is profuse, and has the peculiarly sour smell so often noted in cases of multiple neuritis.

Cerebral and Mental Functions.—Patient lies semi-comatose, as already described, and cannot be roused so as to comprehend questions asked of her. She evinces feelings of pain by a slight cry and a feeble, lethargic attempt to remove the limb from the source of the irritation.

Locomotory System.—There is nothing worthy of note as regards the joints. There are occasional slight jerking movements, especially of the arms.

The patient continued much in the same condition at first, gradually losing ground, then more rapidly after her temperature rose to 102° Fahrenheit. On the night of the 7th inst. her breathing became more difficult, the pulse more rapid and weaker, and she died on November 8th.

It is to be regretted that a more complete history of the patient's condition before admission to the hospital was not obtainable in this case. *Post-mortem examination* by Dr. Leith showed nothing worthy of special note here.

The following organs, &c., were examined microscopically.

The brain and cord, the eleventh and twelfth dorsal posterior nerve roots with ganglia, the right and left sympathetics, the optic nerves; the brachial plexus, the ulnar, median, musculo-spiral, musculo-cutaneous, sciatic, external popliteal, internal popliteal, posterior tibial, and the anterior tibial nerves of both sides were examined along with the extensor communis digitorum, extensor hallucis, and the tibialis anticus muscles; also the phrenic nerves, with diaphragm; the seventh intercostal nerves and muscles; the semi-lunar ganglia; pieces of the ventricular wall of the heart, the liver, &c. The specimens were all preserved in Müller's fluid, and unfortunately the ganglia were put into the same solution.

The motor areas of the brain were examined by the same method as used in Case I., but without any conclusive result.

The cord gave considerable trouble by not hardening properly. The multipolar cells in the anterior cornua show, however, the same changes as those described in Case I. There are no patches of degeneration in the white substance, and there are no hæmorrhages. The medium-sized vessels, especially between the cord and the membranes, show great thickening of the middle coat—the thickening having a hyaline appearance. Anterior and posterior roots are alike normal. The nerves showed the same arrangement of degenerated fibres in groups. The description of the right sciatic will suffice for the larger nerves.

Right Sciatic Nerve.—Many nerve fibres are normal, many show granulation or more advanced segmentation of myelin, and these fibres are mostly arranged in groups. They are associated with the presence of a greater number of segmental and connective tissue nuclei, and also leucocytes. There are some fibres which show loss of axis-cylinders; and, generally, these are in or near the groups referred to. Very few fine fibres are seen to be normal; most appear to be included in these groups. There is homogeneous pink-stained (eosin) fibrinous exudation between many of the fibres. This is most marked in connection with these groups of degenerated fibres, and they bear a distinct relationship to the position of the smaller arterioles and capillaries, from which the exudation may have originated. There are, however, far fewer leucocytes in connection with the exudation than might be expected, and these are mostly seen nearer the degenerated groups and the vessels. The segmental nuclei, especially near the degenerated groups, are greatly distended, and show evidence of proliferation, some of them showing well-marked mitotic figures.

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The connective tissue nuclei have also increased in size and number, but only near the bundles of degenerated fibres. The smaller arterioles and capillaries show very marked enlargement and proliferation of the nuclei of the endothelial cells of the intima; most of the smaller vessels appear almost entirely blocked by these nuclei projecting into their lumen, and the nuclei may be seen in all stages of karyokinesis. These vascular changes, mainly involving the intima, and, to a much less extent, the media and adventitia, are best marked in the endoneurium, and are noticeable also in the perineurium and epineurium, but to a less extent.

The larger arteries in the endoneurium show a marked hyaline change in the media, a structureless-looking coat, often with few nuclei and staining feebly with eosin, and thus strongly contrasted with the fibrous tissue of the intima and adventitia. Arteries so affected measure in total diameter 80μ or thereby: the larger vessels do not show this change, nor do those which are much smaller in calibre than the measurement given. This hyaline appearance of the media is almost entirely confined to the arteries in the endoneurium. The perineurium is somewhat thickened in part, but almost entirely by the increased number of young connective tissue cells in the inner lamellæ of the sheath. Speaking generally, whereas all the funiculi show the vascular changes as regards nuclei, some do so much more markedly than others, and in those in which they are best developed the degenerative changes in the nerve fibres are much more in evidence.

Right Anterior Tibial Nerve.—This nerve is very markedly degenerated. The normal fibres are in little groups, but by far the greater proportion show segmentation of myelin, sheaths distended with droplets of myelin, and axis-cylinders very few in number. Where the axis-cylinders are seen, they extend generally for a short distance alone, and these small portions show typical vacuolation and granulation.

Fig. 4 represents the very advanced segmentation of the myelin in the fine fibres, as compared with the ordinary-sized fibres, and in peripheral neuritis, so far as my experience goes, these fine fibres nearly always suffer at an early date, and very severely.

The nuclei are increased in number, especially those of the connective tissue: there are a few leucocytes near the vessels, and the segmental nuclei are greatly enlarged, and, in not a few instances, proliferated.

The vascular changes are most typically seen in the endoneurium, also in the epineurium. Fig. 3 shows in transverse section the appearances just described, especially the vascular changes and the exudation.

The Right Ulnar Nerve is the last nerve of this case I shall describe here. It is much more markedly affected than the brachial plexus. Two-thirds of the fibres in all the funiculi show degenerative changes by Marchi's method. This nerve presents very different appearances at different levels. The axis-cylinders are much less interfered with than the medullary sheaths, although the latter are considerably thinner than normal. The degenerated fibres in a section which, at first sight, might be mistaken for a healthy nerve, are best marked round the periphery of each funiculus near the septa, and especially near the vessels. Those fibres show distension of sheath and loss of axis-cylinders, and are probably nerve fibres destroyed by pressure higher up, and in which the axis-cylinders may be actually broken across. Those fibres are not more numerous than one-tenth of the total number in the section at the level referred to. Many of the fibres possessing axis-cylinders have an irregular outline and seem somewhat separated the one from the other.

A longitudinal section shows all the appearances at different levels, the localised bulging of the sheath of Schwann by segmented myelin, the effusion between the fibres, and yet many of these very fibres appear above and below with fairly normal axis-cylinders. These changes may depend on vascular conditions, as an effusion is distinctly present along the line of certain of the vessels, and where these vessels run across the funiculus the exudation appears to spread widely. In the gap between the vessels and the neighbouring nerve fibres there are the fibrinous-looking remains of an exudation staining very feebly with eosin, and the marked changes in the medullary sheaths of the contiguous nerve fibres (segmentation, bulgings of wall, etc.), all bear witness to the detrimental effects of the pressure, while the connective tissue and segmental nuclei are enlarged and proliferated, but not very extensively. There are a few leucocytes.

The vessels in the endoneurium show marked nuclear changes, especially those vessels probably causing the exudation—but they are not so evident in the epineurium.

As a good instance of the change in the muscles of the affected limb, I describe the right tibialis anticus muscle.

Right Tibialis Anticus Muscle.—The transverse striation is better marked than could be expected, but many fibres show it

somewhat feebly. There are many nuclei, but these are mostly in the walls of the lymphatics and the capillaries. The sarcolemma nuclei are slightly increased in number, especially those belonging to the fibres which have lost, to some extent at least, their transverse striation. These nuclei may be seen in all the different stages of proliferatien. The capillaries between the fibres are strikingly empty of blood, and many of them are blocked with enormously swollen endothelial nuclei.

These cases are most instructive, because while differing in some respects they yet agree in others.

Firstly.—We have in both cases the same effusion. This effusion is most characteristic; it is found specially around the arterioles and capillaries in the endoneurial septa, often between the nerve fibres and the perineurium, and separating the inner lamellæ of the perineurium. It is not, in these cases at least, a typical inflammatory exudation. In Case I., where in many nerves it is less in amount, it is accompanied by a far greater number of leucocytes than in Case II., where the exudation is much more excessive. This exudation, where referred to at all in most text books on neuritis, is manifestly regarded as an evidence of inflammation, and is not accorded a prominent position. In both cases I have endeavoured to demonstrate that this exudation is greater in one part of the nerve than in another, and that it acts injuriously by compressing the nerve fibres in its neighbourhood, thus causing degenerative changes in them, not merely at the level where it occurs, but also to a greater or less extent peripherally. The exudation tends in time to organise, as many of my preparations show. (See fig. 3.)

Were the exudation to occur in a healthy nerve, it would cause pain, and possibly interfere with the conduction of impulses, sensory or motor, or both; but it could not occasion degenerative changes similar to those seen in my two cases, unless from some cause the nerve fibres were interrupted, or the effusion, becoming organised, had contracted, so as to exert hurtful pressure. But in these cases the stage of organisation of exudation is in its infancy, and the effusion

could, by spreading upwards and downwards, against no very excessive resistance, be prevented from doing any great amount of damage—were it not that the fibres from their condition are specially prone to injury.

Secondly.—In Cases I. and II. the vascular changes were the same; the same increase in size and number of the nuclei of intima, media, and adventitia, limited to the smaller vessels, and first found in endoneurial vessels, and, as the periphery is reached, extending to the vessels of the perineurium and epineurium.

There is no point more difficult to determine with certainty than the existence or non-existence of proliferation of nuclei in vessels. Enlargement can be decided by measurement, but not proliferation. Many of my specimens show, however, mitotic figures in these nuclei, and this seems to me to be sufficient evidence.

I noted in both cases that the effusion, whether with or without many leucocytes, was always accompanied by the vascular change, and that these vascular changes became better marked as the nerves were traced peripherally. Then the larger arteries in the endo- and sometimes the perineurium showed thickening of the middle coat, and the thickening was homogeneous in appearance, and with few nuclei in its structure.

Thirdly.—The condition of the nerve fibres themselves. Taking almost any of the affected nerves, a transverse section shows that effusion and degenerated fibres are in conjunction, whereas the healthier fibres are those least affected by an effusion. Many of the nerve fibres situated near an exudation are greatly distended. Fibres in the presence of an exudation often swell up much as normal nerve fibres do when placed in water; probably it means endosmosis.

But sometimes groups of degenerated fibres are seen without any effusion. This indicates that the fibres are probably pressed upon by an effusion at a higher level. I do not mean to assert that all peripheral nerve fibres degenerate as the result of pressure alone, but this was the principal cause, in the two cases I have recorded.

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To put this point in a different way, I contend that although in alcoholic neuritis the nerve cell suffers chiefly, and with it the process—*i.e.*, the nerve fibre—yet the cell intoxication is not the chief cause of the advanced changes met with more in one fibre in a nerve than in another, but that a local agent—the effusion depending on vascular conditions—greatly expedites the degenerative changes in the previously weakened nerve fibres.

A true parenchymatous degeneration may occur in toxic peripheral neuritis, but there is, in addition, an interstitial effusion which aids in the process, and the greater the effusion the less is the chance of subsequent recovery.

I have noted in several of the nerves the fact that the very fine fibres, which are, I believe, mostly vaso-motor in function, are nearly always markedly degenerated.

Comparing fig. 4, already referred to, we see how much further advanced are the changes in the fine fibres than in those of average size. I believe that it is to the early changes met with in these fibres that the vascular conditions are partly due, but as after nerve section effusion is not the rule, but the exception, and the vascular changes are much less pronounced than in peripheral neuritis, I do not believe that the degeneration of the fine fibres is the cause of the effusion, but only aids in the nuclear changes in the arterioles, capillaries, &c. The changes in the axis-cylinders are well shown in both cases, granulation, swelling, etc.

Fourthly.—I found in Case I. the diminution and alteration in shape of the nuclei in the multipolar cells of the most affected side of the grey matter of the cord, and in some cells on the other. Had all the cells shown the same characteristics I might be compelled to admit that the condition was artificial, but the contrast between the two sides, as shown in my photograph, is too marked to allow of such a conclusion. The fact that the cortical cells in Case I. agree with Berkeley's observation on rabbits is of interest, suggesting that these cells are also affected.

In Case II. the electrical reactions compare in a remarkable way with the pathological descriptions, and it is specially interesting to note that an extraordinarily powerful

Faradic current had to be used before contraction of the muscles under examination were obtained.

In conclusion, in Case I. there is, with less exudation, less change in myelin and axis cylinder, and less marked change in vessels, the same amount of paralysis as in Case II., which shows more exudation, more segmentation of myelin and destruction of axis-cylinders and greater vascular changes; therefore in Case I. we have probably a much more advanced change in the central cells than in Case II., and the inference is that, but for the effusion, Case II. might possibly have survived much longer.

In connection with these two cases just contrasted together, I should like to note the similarity of changes in the right posterior tibial nerve from a case of diabetic neuritis, a male, aged 36, who died from a pulmonary complication, and not of diabetic coma. I am indebted for the specimen to Dr. Alexander Bruce, of Edinburgh.

Case of Diabetic Neuritis—Right Posterior Tibial Nerve.

Many fibres appear normal. These are mostly seen in groups, and the groups form a more or less irregular pattern in the funiculi. Between these groups of fibres those that are degenerated appear also grouped together. The myelin is extremely granular and the neurolemma tends in certain fibres to give way. The axis-cylinders of affected fibres are distended, sometimes showing regular bead-like swellings, granular, often vacuolated, and stain feebly. Among these groups of degenerated fibres many nucleated connective tissue cells appear. Some of these show greatly distended nuclei with a very small amount of protoplasm, the protoplasm staining deeply with eosin. They show evident traces of proliferation, and it is possible that by their presence they may exert a prejudicial influence over the nerve fibres in their neighbourhood. Segmental nuclei are not markedly proliferated, although they are distinctly enlarged in size. The exudation described in Cases I. and II. appears here

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also, being specially well marked between the perineurium and the nerve fibres and along the lines of the septa.

The vessels show the most typical enlargement and proliferation of the nuclei of the endotheliel cells of the intima; to a less extent the nuclei of the media and adventitia are similarly affected. The smaller vessels suffer most, although every vessel in the endoneurium appears affected, the arteries much more markedly than the veins. The vessels of the perineurium are involved, but to a less extent. In some of the larger arteries a local proliferation of the endothelium seems to have occurred, the process thus formed suggesting an appearance not unlike little endothelial buds. This appearance suggests the possibility of these little processes becoming detached and forming emboli in the minute capillaries. There are certainly little cells apparently free in the blood stream which are somewhat suggestive of free endothelial cells.

The appearances noted in the muscles of Case II., especially the left tibialis anticus, strongly suggest that these little emboli do occur.

Ascending Degeneration in Nerves.

A summary of the results of my experimental work on dogs and rabbits, and the observations made on the ulnar nerve of a stump ten years after the amputation of the arm, were given at the British Medical Association meeting in Carlisle last summer, and are to be found in the October number of the journal, and they appear in a more extended form in the January number of the *Edinburgh Medical Journal*. I shall, therefore, only give my conclusions in the briefest possible way.

In the ulnar nerve of the patient referred to, the fine medullated fibres have suffered most; they are markedly compressed by connective tissue. These fine fibres, which may be easily seen occurring in little groups, are most affected close to the terminal neuroma. The arterioles and

capillaries in the nerve (endo- peri- and epineurium) show the nuclear changes described in peripheral neuritis, but the changes become less obvious as the nerve is traced centrally. My inferences are that the fine fibres whose function is lost are replaced by connective tissue; that those fibres which have so lost their function become proportionately less numerous as the nerve is traced centrally; and that the nuclear changes in the vessels depend on the minute fibres whose function is not abrogated being compressed by the connective tissue replacing minute fibres whose function is abrogated. In other words, I believe that fine fibres are chiefly vaso-motor, and mainly suffer in ascending degeneration. The connective tissue increase around the larger fibres is extremely slight in proportion, even around fibres which must have lost their function. I should note that corresponding vascular changes occur in the muscles, &c.

In a large series of experiments on dogs and rabbits, mainly the latter, I find similar results.

Figs. 5 and 6 show longitudinal sections of the two sciatics from a rabbit. Fig. 5 is the normal nerve, and the fine fibres can be seen about the centre of the funiculus, forming a distinct strand. Fig. 6 is from the central end of the left sciatic twenty-three days after a double ligature was applied to the nerve. The well-marked connective tissue septa mark the site of fine medullated fibres, all of which suffered severely from the pressure, and there is little, if any, connective tissue increase elsewhere.

This change begins about the fourth day, and is observable in a week's time after operating; it occurs whether sections or ligatures be made, and is found in dogs and rabbits alike. The arterioles and capillaries show corresponding changes to those found in the human ulnar nerve after amputation. In short, the changes found in the central ends of rabbits' sciatics after section or ligature appear closely to correspond to those in the nerve of the stump in man. I have had the opportunity of corroborating these observations on the nerves of human stumps at different dates, but have still an insufficient number of cases at my disposal.

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Descending Degeneration in Mixed Nerves, after Section or Ligature.

It is my intention only to refer to those changes which have any bearing on peripheral neuritis.

(1) Many nerve fibres in the peripheral end of a divided nerve in dogs and rabbits are distended. This distension, best marked within a few days after the operation, may be due to aggregation of myelin droplets; but where there has been an exudation from neighbouring vessels the fibres may swell up, probably from inhibition. This appearance is of very common occurrence in peripheral neuritis, although accidental in experimental sections.

(2) I do not propose to discuss at any great length the changes observed in the axis-cylinder. I have noted a granular, vacuolated, or distended appearance very commonly in the peripheral end of a divided nerve up to such a time as axis-cylinders are easily observed.

The life of an axis-cylinder in a severed nerve is of great interest. Observers state that they ought to be become unrecognisable in three to four weeks, and disappear altogether (*i.e.*, are unstainable) in a period not exceeding six weeks. In the rabbit, the peripheral end of the axis-cylinder remains for a longer time nearly normal in appearance after ligature than after section, just as segmentation of myelin and proliferation of segmental nuclei are slower in taking place; but, whatever happens, an axis-cylinder appears to derive trophic influence of some nature from the segmental cells.

An axis-cylinder may be seen swollen and distended in a fibre with apparently normal myelin, although the distension might be artificial. An axis-cylinder may be healthy, or apparently so, where myelin is granular or broken up (to a very limited extent), but an axis-cylinder is rarely, if ever, found to be normal in function or appearance where segmental nuclei are proliferated. It is difficult to bring forward any photographic proof of this, but a careful study of many sections of many specimens gives strong corroborative evi-

dence in favour of the truth of this theory, however unlikely it may seem from an embryological standpoint.

(3) The change in the vessels of a nerve undergoing Wallerian degeneration is described as consisting in enlargement and proliferation of endothelial nuclei of arterioles and capillaries, and slighter nuclear increase in media and adventitia. Venules are less affected. I would only here point out that these changes are not nearly so well marked in most of my experimental sections and ligatures as in peripheral neuritis, and in many cases not marked at all.

(4) The question so much discussed as to where the degeneration process begins in the peripheral end of a divided or ligatured nerve is a very difficult one to answer. I made a large series of experiments on rabbits with double sections and double ligatures, so as to observe whether the middle or peripheral portion of the nerve degenerated first. The operations were done without displacing the nerve and as long a middle portion was left as possible between the two sections or ligatures.

Notwithstanding that the middle portion is at a disadvantage, because the blood supply is presumably more interfered with, the balance of my evidence is in favour of the peripheral part commencing to degenerate first.

In one rabbit, for example, in the middle part the segmental nuclei have not proliferated to nearly the same extent as in the peripheral. In another rabbit, 23 days after double ligature, the segmental nuclei have evidently only just ceased proliferating, whereas the process had long ceased in the peripheral. After four days ligature or section the degenerative process appeared to commence all along the severed nerve, but to be most advanced in the peripheral part, with the exception of certain fibres in that part which long retained a fairly normal appearance, though they also eventually suffered.

I cannot, however, leave this subject without referring again to fig. 4, which is taken from the right anterior tibial nerve of Case II. It shows what is likewise well seen in the peripheral ends of the divided or ligatured nerves—namely, that the fine fibres suffer most severely. In the photograph

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they may be seen markedly segmented. Their medullary sheaths show far more advanced degenerative changes than the ordinarily-sized fibres. In only one of the whole series of experiments on rabbits was the evidence not corroborative.

I propose next to contrast peripheral neuritis with descending Wallerian degeneration.

If a nerve fibre be severed completely from its centre, it undergoes Wallerian degeneration, but in peripheral neuritis the fibre below the level of the degeneration is apparently normal. There may be several such degenerated patches in the same fibre, with intervening and almost healthy-looking portions, although it is not likely that the peripheral end organs, whether sensory or motor, could be entirely, if at all, responsible for these healthy intervening portions.

The obvious conclusions are, that the fibres which show these alternating changes are still nourished by the cells from which they arise, and that the local changes are due to a locally injurious, or toxic, agent acting only in a limited area.

It has been stated that these degenerated tracts are at points where the nerves are exposed to external injury or pressure, for example, the ulnar nerve at the elbow, but this is far from being a complete explanation of the difficulty.

These degenerated patches have an infinitely wider range than merely points of possible external pressure; they are more marked peripherally than centrally, but, what is of much greater importance, these changes bear a distinct relationship to the condition of the vessels.

Wherever an exudation is well marked the vessels show most distinctly enlargement and proliferation of nuclei, and the hyaline-looking thickening of the middle coat of the endoneurial arteries. Why may not these conditions be due to the toxic action of a poison on the vaso-motor cells in the medulla? It cannot be solely due to this, or else all the vaso-motor nerves would be simultaneously interfered with, and it is undoubtedly true that in Cases I. and II. the vascular changes varied greatly in the different nerves, as the pathological specimens demonstrate. But the toxic agent may act locally as well as centrally. An injury to a nerve, as, for example, a squeeze or any local irritant, may damage

the fine fibres, which are probably vaso-constrictor, much more markedly than the larger fibres; and in any of the cases of peripheral neuritis which I have had the opportunity of examining, these fine fibres, arranged in groups, seem to be the point where the earliest and most marked proliferation of connective tissue nuclei is observable.

Each individual fibre obtains its nutritive supply from lymph or blood. The toxine in the blood will act with great virulence upon these fibres brought into immediate contact with it. The nerve fibre may be deriving nutritive material, more or less throughout its whole length, whether by means of segmental cells or not we cannot say; still there seem to be certain points along its course where it comes more directly under the influence of the blood current. The fact of the existence of the nuclear changes may be due to the lymph surrounding the fibre being to a certain extent toxic, and specially at the points where a further nutritive supply is derived from the capillaries, and where the greater toxic effect may be produced. This may cause an effusion at the level of distribution of the fibres to the nerve vessels. When a bundle of fibres shows degenerative changes at one level it shows similar changes, not merely repeatedly as the periphery is approached, but with shorter and shorter intervals of more normal nerve fibre between.

Changes in Nerve Cells. Ganglia on posterior nerve roots and multipolar cells in anterior cornua. (Rabbits and Dogs.)

After a very considerable amount of experiment, I found that a solution of corrosive sublimate (saturated in a 0·75 per cent. solution of common salt) with equal parts of water, heated to the temperature of the tissues, was the most satisfactory fixing agent for the nerve cells of rabbits' ganglia and cord, and I succeeded in getting fairly satisfactory nuclear networks (as compared at least with my earlier specimens) and almost no evidence of cell shrinkage. I had control nerve cells to compare with those which were abnormal. The stains used were toluidin blue and eosin, a method for which I was originally indebted to Dr. Gustav Mann.

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Quoting from my paper, read before the British Medical Association at Carlisle, the changes I obtained were—

(1) The cells of the ganglia on the posterior nerve roots undergo definite changes as the result of nerve section or ligature, and do so at a much earlier period than the multipolar cells in the cord—beginning probably as early as the fourth day, and certainly by the seventh day.

(2) That one of the very first changes observed in the cells of ganglia and anterior cornu is a diminution in the size of the nucleus in proportion to the size of the cell, and that sometimes, but not in all cases, nucleoli also become smaller, and very frequently the nuclei take up an eccentric position, sometimes even bulging the cell wall.

(3) That in both sets of cells Nissl's granules, otherwise known as the chromatic granules, are either smaller in size, fewer in number, and scattered through the cell body tending to be most numerous round the nucleus, or else they are grouped together in large masses round the nucleus, leaving the periphery of the cell quite clear.

(4) That pericellular lymph spaces may become enlarged, especially around the ganglia cells, and where the enlargement is very marked the cells become proportionately smaller in size, although an actual atrophy may also occur. In several of my specimens I found large vacuoles—not the vacuoles described by many writers as occurring in the cells of the cord and cerebral cortex, which are probably to some extent artificial—but big vacuoles, more resembling hugely distended pericellular lymph spaces. They differ, however, inasmuch as they are surrounded by the remains of cell protoplasm containing chromatic granules.

(5) That in the multipolar cells not merely are there these changes in position and size of nuclei, and arrangements and number of chromatic granules, but there is, as a later phenomenon, marked disintegration of cell protoplasm, well seen in some of my specimens. This disintegration has been described by Marinesco as occurring in certain cord lesions in man. It consists of patches, which, with toluidin blue and eosin, are whitish in colour and surrounded by masses of chromatic granules.

This stage of disintegration follows only at a very late stage in the ganglion cells of the posterior nerve roots. It should, however, be stated that a varying number of normal cells occur in abnormal ganglia, and that a very few abnormal cells occur in normal ganglia. The changes in the cells most commonly observed in normal ganglia are an aggregation of chromatic granules round the nucleus, and more rarely in eccentric position of nuclei. By normal ganglia I mean ganglia of presumably healthy rabbits never submitted to any experiment at all, as well as ganglia unaffected by the experiments performed. The proportion of such abnormal cells in a normal ganglia rarely exceeds 2 per cent. of the whole. Vacuoles may also appear, but much more rarely, and their presence is quite exceptional.

Figs. 7 and 8 show the changes described in the ganglia on the posterior nerve roots—7 is the normal, 8 the affected side twenty-one weeks after double ligature of the sciatic. The ganglia are a pair belonging to the sciatics, and the operation in this case was successful in every way as regards healing by first intention and the subsequent health of the animal.

Fig. 9 shows the multipolar cells from the anterior cornua of a rabbit six weeks after a double ligature had been applied to one sciatic. The cells are taken from the same level of the cord in the "sciatic" region. The affected cells are to the left side of the photograph.

Tables of measurements showing the difference in size of neuclei and cells in the affected and normal sides will, I anticipate, be published in the March number of the *Edinburgh Medical Journal*. I may state, however, that with very few exceptions, in my forty to fifty experiments on rabbits, the changes recorded appeared in the affected side, and that the exceptional cases referred to were some two or three in number, in all of which the wound had become septic.

It seems quite comprehensible that the cells of the ganglia in the posterior nerve roots should suffer first, because nerve impulses pass normally up to them from the site of the lesion; whereas the cells of the multi-

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polar cells in the anterior cornua normally send impulses downwards from them, although there is no question that impulses can pass in both directions along a motor nerve fibre.

Are the connective tissue changes along the lines of the fine medullated fibres in the central end of a divided nerve and the changes in the nerve cells of ganglia and cord due to irritation? This is a very difficult question to answer. I do not believe irritation from the site of the lesion is alone the cause, because very different results were obtained on examining the nerve cells in septic cases; and further, the rabbits never showed any signs of pain, eating a hearty meal whenever they recovered from the anæsthetic, and feeding regularly and well till the day of their death. I am about to try to eliminate still more the possible irritation factor in producing "ascending degeneration" in a new series of experiments, as irritation may not always be evidenced by pain.

The suggestions which may be culled from comparing the cases of peripheral neuritis and the nerve degeneration, and the changes in nerve cells as the result of experimental section or ligature, are:—

The fine medullated fibres, which appear to play such an important *rôle* in "ascending degeneration," degenerate very early in peripheral neuritis, and are associated with marked vascular changes.

The comparative absence of the vascular changes in Wallerian degeneration is evident from an examination of my series of specimens.

The limited nature of the vascular changes in the central end of the divided sciatic in dogs and rabbits, and in the nerves of stumps in man, suggests not merely the relationship of fine medullated fibres to the vessels, but is also a strong contrast to the great vascular changes found so frequently, at least, in peripheral neuritis.

Peripheral neuritis is different from either ascending or descending degeneration, and is manifestly caused by a toxine acting on cells as well as nerve fibres, and possibly vessels.

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The fact that in Case I., and also Case II., the multipolar cells showed the same changes that appeared after experimental section (so far as the methods of fixing would permit of comparison), is most suggestive, and I would draw especial attention to the diminished size of the nuclei in the affected cells, a point I have not seen noted by other observers.

The changes which I found in the cells of the ganglia on the posterior nerve roots after a nerve section or ligature appeared at such a very early period, much earlier in fact than in the multipolar cells, that they suggest the theory that the division of an axis-cylinder process acts more rapidly in a prejudicial manner on that cell to which the process normally conducts nerve impulses.

This would naturally suggest that if the multipolar cells and ganglion cells were affected by a toxic agent, the processes normally conducting *from* the cell would suffer more rapidly than the processes normally conducting *to* the cell.

My results show that, up to a certain date after section or ligature, the ganglia will suffer most organic change, but that after three weeks the multipolar cells rapidly undergo alteration, and that in six to seven weeks, according to Marinesco, Golgi, and others, disintegration of protoplasm occurs. My results, however, only show distinct disintegration of protoplasm of the cells of the posterior nerve root ganglia after fifteen to twenty weeks, although it is well marked in multipolar cells in three to seven weeks. May not this explain why, in peripheral neuritis, long after paralysis is almost complete, sensation to pain may persist, to some extent at least?

In peripheral neuritis, the condition of these ganglia cells have not met with much attention, "vacuolation" and "hyaline swelling," &c., being referred to in only a few cases; but I trust to be able in a few months to have some work completed upon the condition of these cells, in one case, at least, of this disease.

In conclusion, I may state that I have been engaged upon the histology of these fine fibres, and their distribution, etc., at different stages of life, in human nerves. The results are

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as yet incomplete, but this much may be asserted: that the connective tissue supporting the fine fibres becomes gradually increased as age advances, and that diseases causing arterial degeneration appear to expedite the process.

The methods used in the microscopical work were numerous; nerves were, however, always fixed in Müller's fluid, and portions of cord, etc., in corrosive sublimate. Hæmatoxylin and benzo-purpurin were mostly employed for staining nerves, in addition to Weigert-Pal's method, and toluidin blue and eosin for nerve cells. I cannot close this paper without expressing my thanks to Sir Thomas Grainger Stewart and Dr. George A. Gibson for permission to give details of the two cases of peripheral neuritis which were under their care, and by whose courtesy I obtained the specimens described in the earlier part of this paper.

DESCRIPTION OF FIGURES.

FIG. 1.—Multipolar cells from the anterior cornua of the cervical enlargement of the spinal cord of Case I.

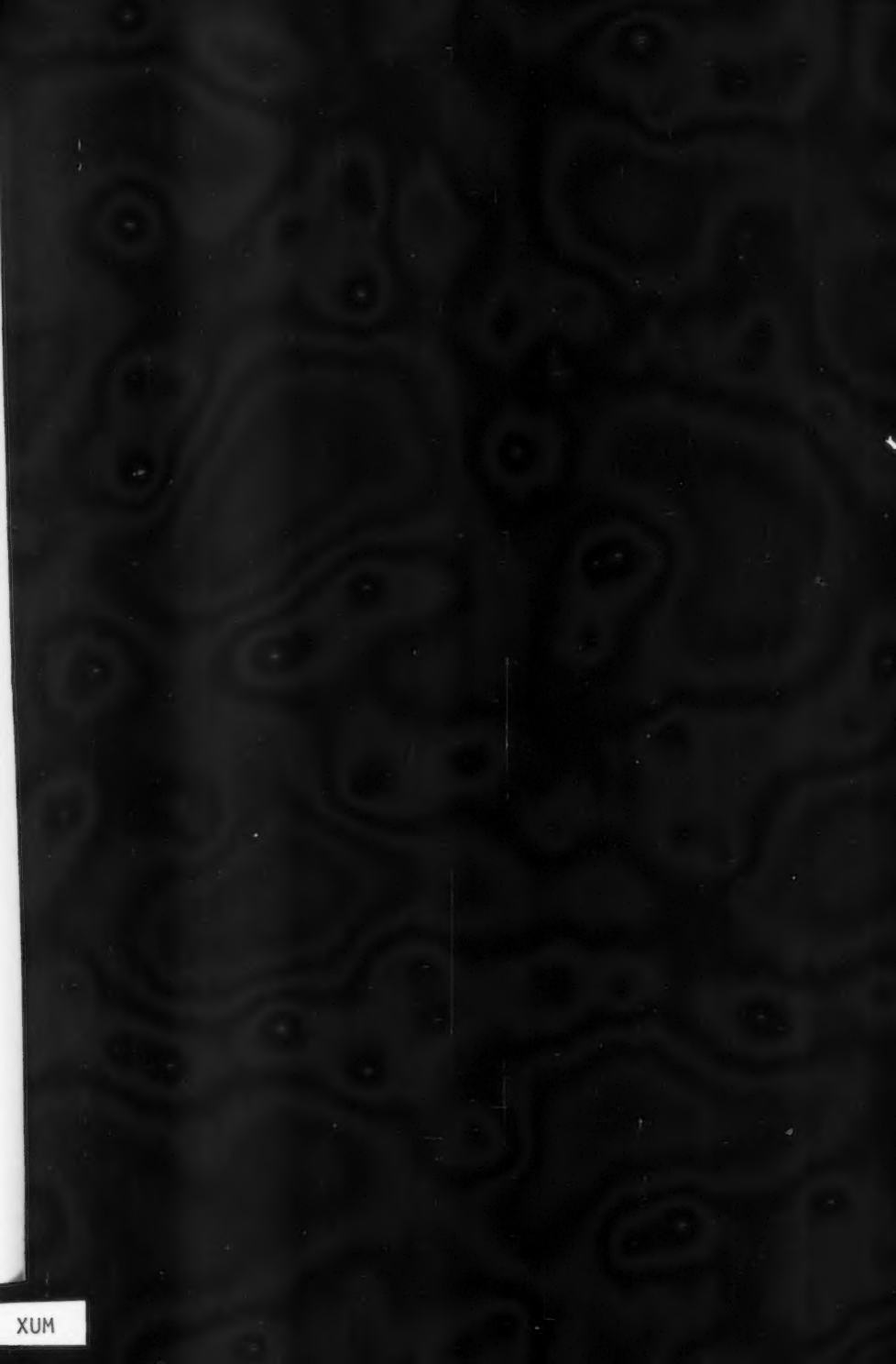
a. = Cells from left anterior cornu.
b. = Cells from right anterior cornu.
mc. = Multipolar cell.
nu. = Nucleus.
nl. = Nucleolus.
enl. = Endonucleolus.

FIG. 2.—Right sciatic nerve, Case I., showing recent leucocyte exudation around arteriole.

arl. = Arteriole.
l. = Leucocytes.
ex. = Exudation.
n. = Normal nerve fibre.
nr. = Degenerated nerve fibre.

FIG. 3.—Left sciatic nerve, Case II., showing exudation, with commencing organisation, and the change in coat of arteriole.

arl. = Arteriole.
ex. = Exudation.
fn. = Newly-formed connective tissue fibres.
en. = Endothelial nuclei.
me. = Media.
ad. = Adventitia.
adn. = Nuclei of adventitia.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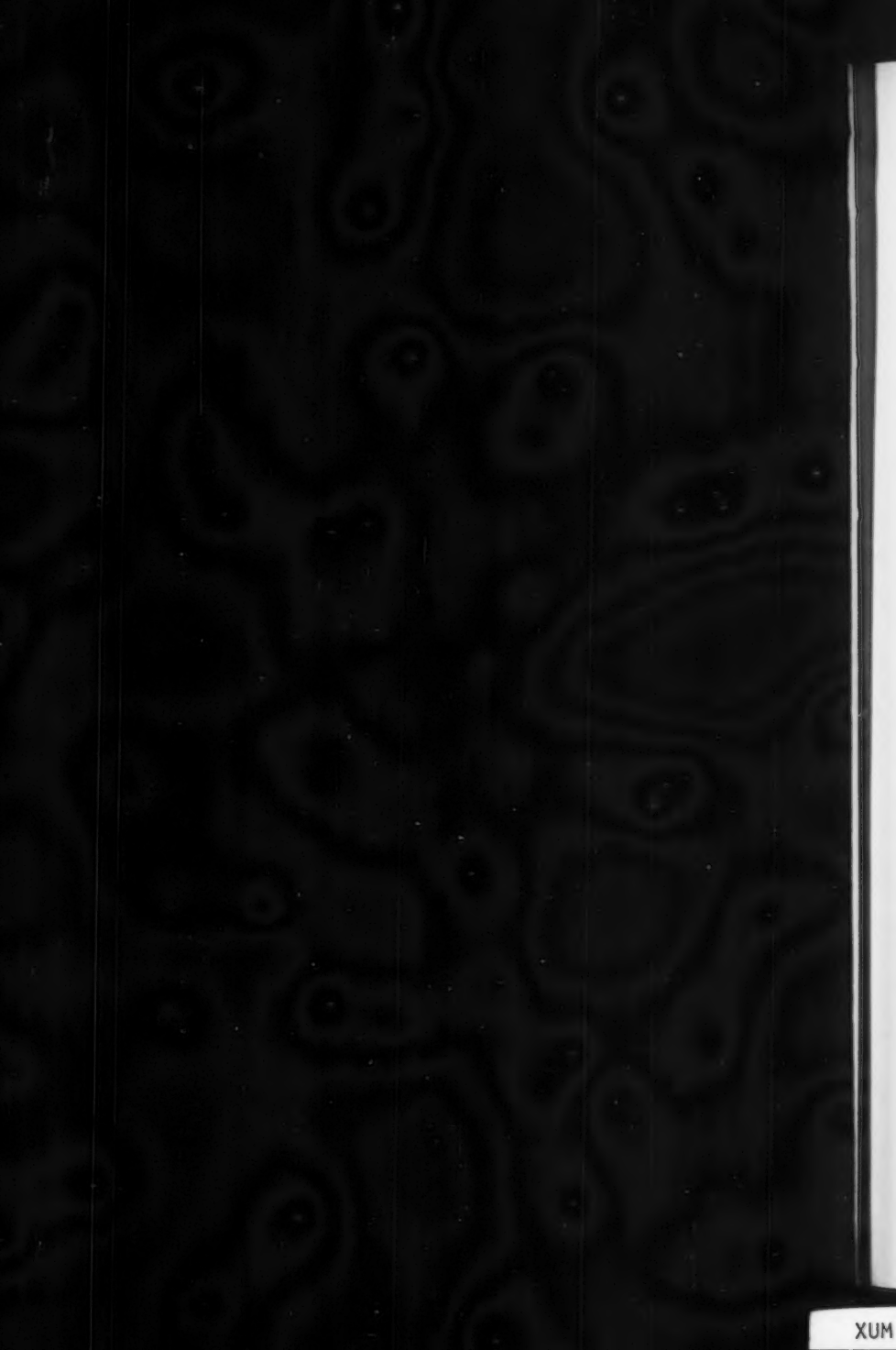
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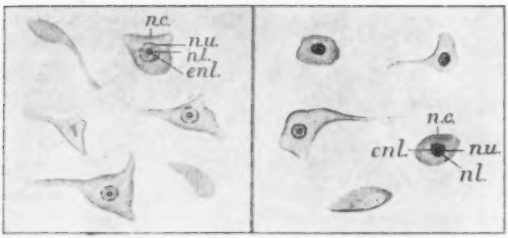

a. *b.*

FIG. 1.

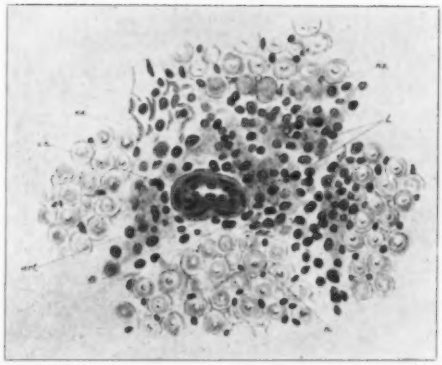

FIG. 2.

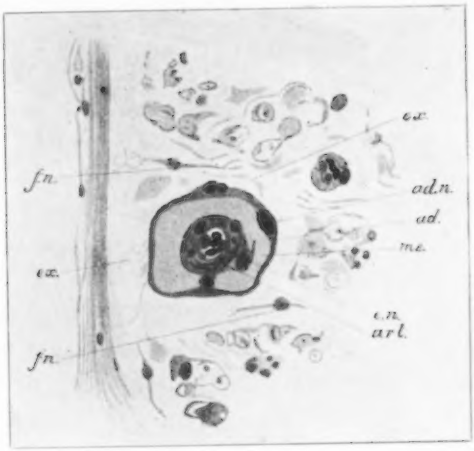

FIG. 3.

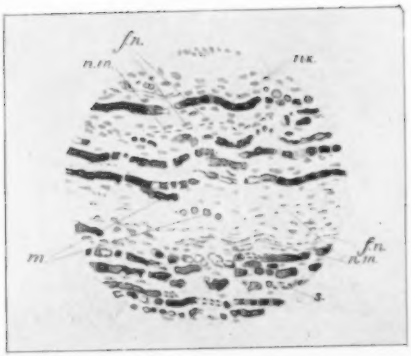

FIG. 4.

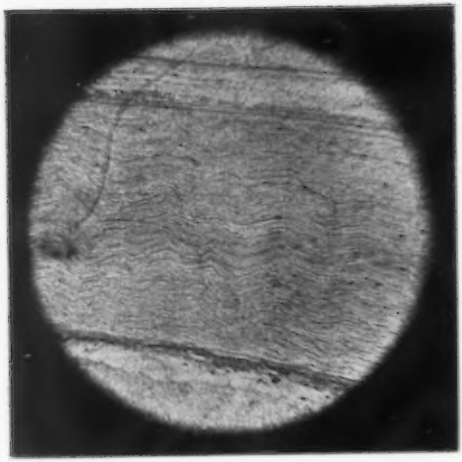

FIG. 5.

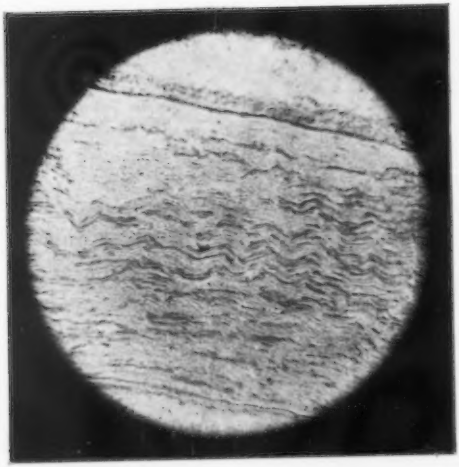

FIG. 6.

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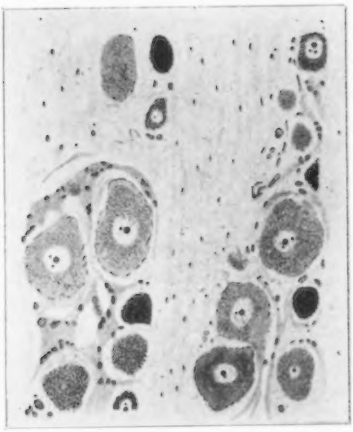

FIG. 7.

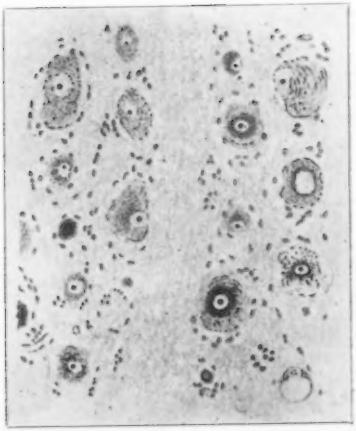

FIG. 8.

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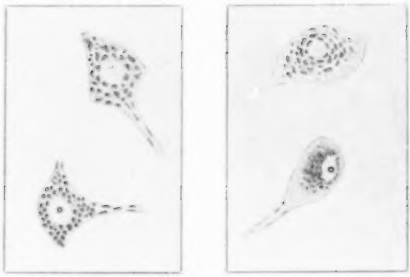
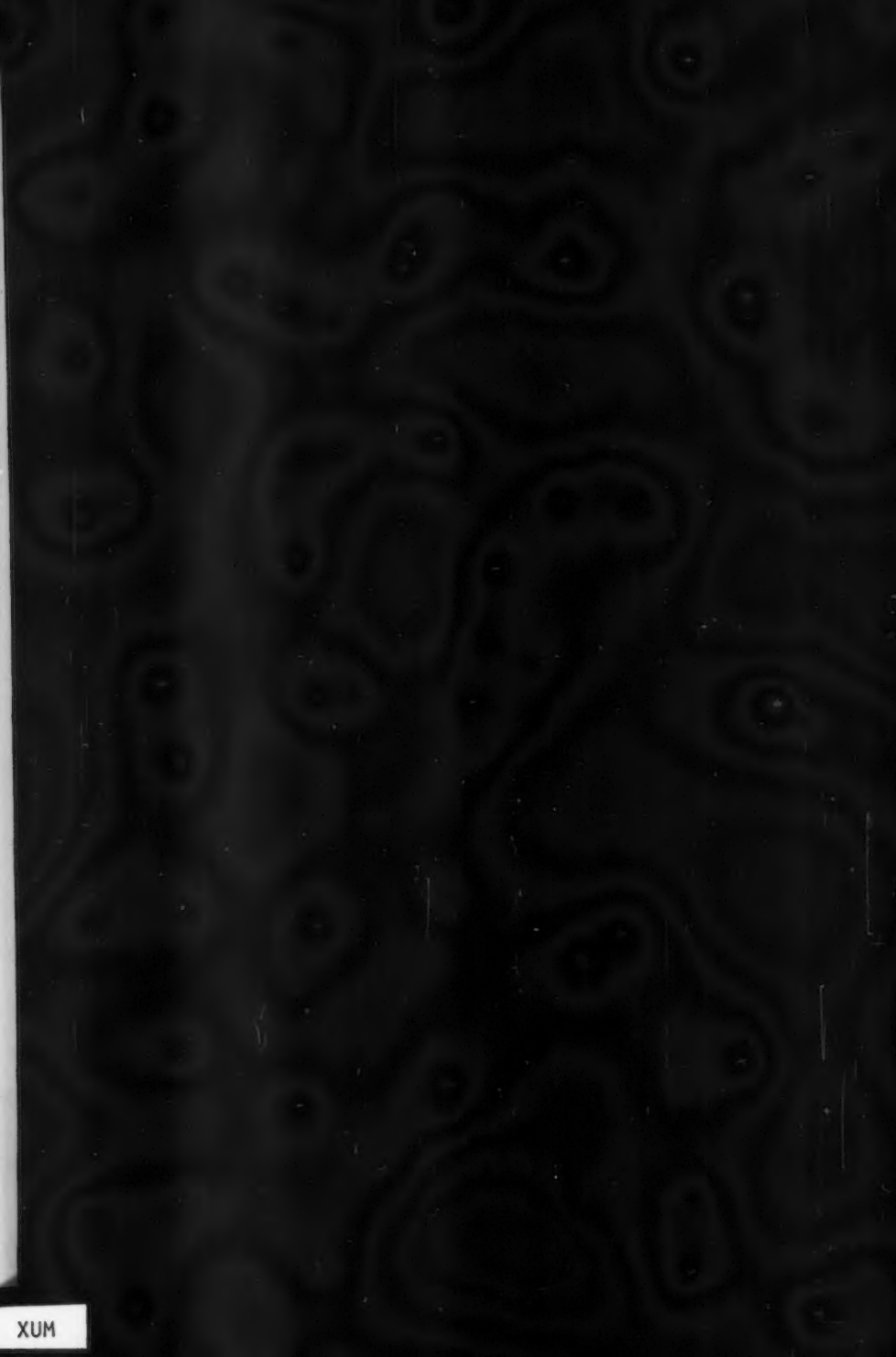


a. *b.*

Fig. 9.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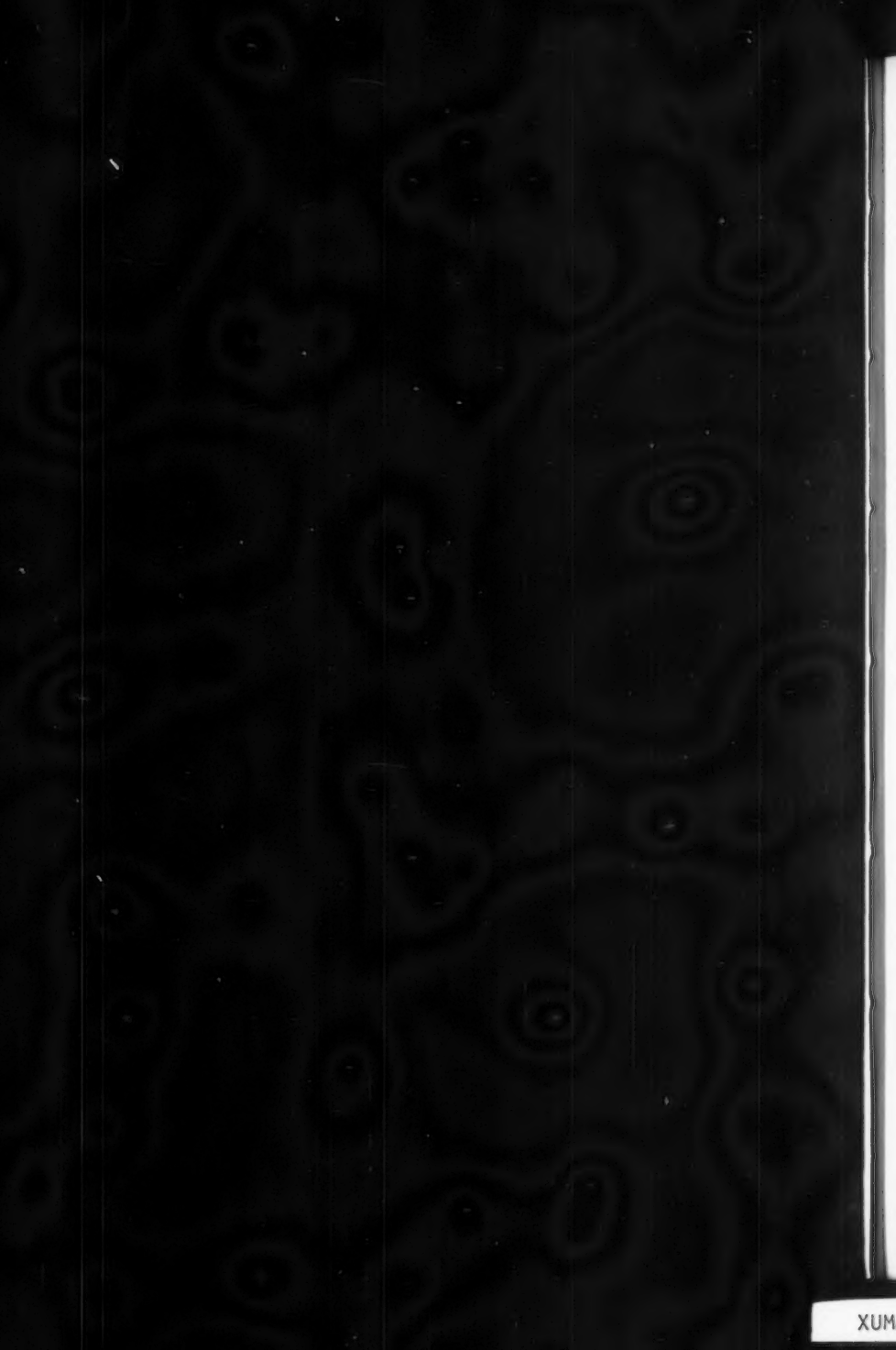
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FIG. 4.—Right anterior tibial nerve, Case II, showing greater segmentation of fine than of average-sized fibres.

nm. = Degenerated fine fibres.
fn. = Proliferated connective tissue in relation to these fibres.
s. = Enlarged segmental nucleus.
nx. = Degenerated nerve fibres—average size.
m. = Myelin droplets.

FIG. 5.—Right sciatic nerve of rabbit—unaffected side—showing fine medullated fibres in groups or strands.

FIG. 6.—Left sciatic nerve of rabbit—central end, twenty-three days after application of a double ligature—showing thickened connective tissue septa replacing fine medullated fibres.

FIG. 7.—Ganglion on right posterior nerve root (sciatic region) of rabbit—unaffected side—stained with toluidin-blue and eosin.

FIG. 8.—Ganglion on left posterior nerve root (sciatic region) of rabbit—twenty-one weeks after double ligature—stained with toluidin-blue and eosin.

FIG. 9.—Multipolar cells from anterior cornua (sciatic region) of rabbit's cord.

a. = Normal side.
b. = Cells from affected side, six weeks after a double ligature had been applied to sciatic nerve.

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TWO CASES OF PORENCEPHALY.

BY J. WIGLESWORTH, M.D.LOND., M.R.C.P.

Case 1.—Joseph B., aged 24, single, was first admitted into Rainhill Asylum in March, 1890. He was the youngest of five children, all of whom were living and healthy. His parents were living, his father being 60 years of age and his mother 64, their respective ages at marriage having been 25 and 29; there was no consanguinity. His father was intemperate, and was said to be of an excitable temperament, but, with this exception, there was nothing of note in the family history, and, in particular, there was no evidence of epilepsy or other form of nervous trouble. The patient himself had smallpox when 3 months old, and had been delicate since; he did not walk until he was 4 years of age; went to school at 6, and remained until he was 14. He was said to have been intelligent and bright up to the age of 13 years, but he did not advance after that; he was tried at office work at 14, but failed in memory, and had to give it up. At 15, epileptic fits supervened, and he gradually became more dull; he assisted, however, in his father's shop up to the age of 23; the fits, however, became worse, and during the year previous to admission he became maniacal and unmanageable after them, and had at length to be placed under restraint. There was at no time any history of injury.

The patient, on admission, was a young man of fair physical development, 5 ft. 6 in. in height, and weighed 10 st. 2 lbs. Regular features; brown hair; grey irides; pupils, 4 mm., equal, regular, reacted normally to stimuli. Deep reflexes exaggerated. No definite paralysis, but he had a little weakness of the right hand, and he used to wear a leather band round his wrist to support it. Thoracic and abdominal viscera normal.

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He had a marked stammer in his speech. No sensory defect observed. He had an intelligent expression. He was rational and coherent in conversation, and able to give a very fair account of himself, and quite realised his position; he was, however, evidently rather simple, and there was general slight obtuseness of his mental faculties, with some impairment of memory. He was very fanciful and hypochondriacal, constantly complaining about trivial matters and imaginary ailments, after the manner of epileptics generally; and at times he was excitable, and disposed to be impulsive and aggressive. He had a large number of fits, especially at night. His mental condition might, indeed, be described as one of moderate imbecility. He improved somewhat under treatment, and in December, 1890, his father was allowed to take him out. He again, however, proved unmanageable at home, and was brought back to the asylum ten months later, viz., in September, 1891. His condition then had not altered materially, but he was somewhat more dull. His fits continued numerous, and these frequently assumed a hysterical character, with apparently purposive movements. During the year 1892 he had 453 fits; during 1893 he had 226; and 152 during 1894; of the last-named number, 47 occurred during an attack of status epilepticus in the month of May. In 1895 the fits again increased to 239 in number. In February, 1896, he was again seized with an attack of status epilepticus, which proved fatal in five days, after a series of 54 fits of a very severe character.

Autopsy (*Fifteen Hours after Death*).

An examination of the head only was permitted.

Cranium.—Skull symmetrical externally, somewhat increased in thickness and density, the left side being on an average 3 mm. thicker than the right; sinuses and venous system generally gorged with blood; a general absence of fluid in the subdural space. Pia-arachnoid somewhat increased in thickness, the vessels of the pia being intensely congested. Thinly scattered over the surface of the convolutions in both hemispheres were some small circular, slightly-elevated nodules, about 1 mm. to 2 mm. across, which, for the most part, came away with the pia when this was stripped, and consisted chiefly of fibrous tissue. The whole brain was much congested, the grey matter of the cortex and basal ganglia being everywhere dark, and the consistence was firm.

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Weights.

Brain (immediately after removal)	=	1,372 grammes
Right hemisphere (unstripped)	=	662 ,,
,, ,, (stripped) ...	=	640 ,,
Left ,, (unstripped)	=	539 ,,
,, ,, (stripped)	=	515 ,,
Cerebellum	=	148 ,,
,, Right half	=	69 ,,
,, Left half	=	79 ,,
Pons and medulla	=	32 ,,

Right Hemisphere.—Convolutions large and well formed, and these and the main sulci follow a typical course. No focal lesion. In fact, with the exception of the general congestion, due to the mode of death, this hemisphere was perfectly normal.

Left Hemisphere.—This presented, at first sight, nothing particularly abnormal (except the general increase of vascularity above mentioned), but when the membranes were stripped it was found that these covered, and for the most part concealed, a considerable cavity which, roughly speaking, occupied the upper part of the ascending limb of the fissure of Sylvius, and which had been formed by the removal of considerable portions of the convolutions bounding this area. Strictly speaking, indeed, it was scarcely correct to talk of cavity formation, as before the removal of the membranes the surrounding parts were so nearly in apposition that the aperture which had existed was almost obliterated, so that there was no cystic appearance, and it was not until the lobes were separated from each other that the considerable loss of substance which had occurred was clearly apparent. The missing portions are as follows:—At the upper part, the lower fourth of the ascending frontal and ascending parietal convolutions, together with the anterior half of the supra-marginal gyrus have completely disappeared, whilst inferiorly the temporo-sphenoidal lobe is divided by a deep fissure situated at about the junction of the middle with the posterior third, which cuts through all the three external convolutions of this lobe to a depth of about 1½ cm.; the fissure is 2 cm. broad at its upper end where it traverses the first temporo-sphenoidal gyrus, and tapers off gradually to a breadth of 1 cm. at the third temporo-sphenoidal gyrus; the adjacent long convolutions of the island of Reil, which run into the temporo-spenoidal lobe have also disappeared. The lower ends of

the two central gyri have a slightly eroded appearance, but the ends of all the other defective convolutions where they abut on the cavity are neatly rounded off, and there is no induration round the margins of the cavity. There is no communication with the ventricle. Round the hollow thus formed the different sulci and gyri (especially those at the posterior part of the hemisphere) are grouped in a distinctly radiate manner, so as greatly to obscure the normal arrangement, and make recognition of the different parts somewhat difficult. By tracing up, however, the callosomarginal sulcus on the inner aspect of the hemisphere (where the convolutions follow a normal course) it is possible to locate accurately the fissure of Rolando, and it is thus found that this fissure is not only situated further back than normal, but that it runs in an almost vertical direction from above downwards, its lower end terminating in the cavity above described; it is also shorter than normal, owing to the loss of substance of the two limiting convolutions, ascending frontal and ascending parietal, which in addition to the loss of their lower fourths, are reduced in size throughout their remaining lengths. Behind the fissure of Rolando run a series of deep fissures from before backwards, spreading out, roughly speaking, like the branches of a fan from the cavity above-mentioned, and the convolutions bounding these fissures have, of course, a similar arrangement. Immediately behind the fissure of Rolando, and almost parallel with it, is a fissure which appears to represent the anterior half of the intraparietal sulcus, or post-central sulcus, running in a nearly straight direction from above downwards; and behind this, the ascending limb of the fissure of Sylvius runs in a sloping direction, almost up to the margin of the hemisphere; behind and below this again comes a deep fissure, obviously the posterior part of the first temporo-sphenoidal or parallel sulcus, which cuts right through the parietal lobe, and almost joins the parieto-occipital fissure. Towards the lower and outer margin of the temporo-sphenoidal lobe, the third temporo-sphenoidal sulcus runs back as a deep and well-marked sulcus from the fissure cavity in this lobe, almost to the tip of the occiput; whilst between this sulcus and the first temporo-sphenoidal one, above-mentioned, two more deep sulci run backwards from the fissure-cavity to the occiput in an approximately parallel direction, one of which is, however, interrupted by a bridging convolution; these two latter sulci appear to represent a re-duplicated second temporo-sphenoidal sulcus. All these temporo-sphenoidal sulci run directly into the fissure-cavity which cuts across the temporo-sphenoidal lobe and appear to terminate

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in it, the first one alone having an apparent continuation on the opposite (anterior) side of the lobe; on the anterior side of this lobe, however, two other short but deep sulci curve upwards and backwards over its lower margin as if attempting to meet those on the opposite side of the fissure-cavity.

The first temporo-sphenoidal gyrus is much reduced in size, but the other convolutions of this lobe appear to be of average depth and size, except of course where their structure is destroyed by the fissure-cavity which cuts across them.

Turning now to the frontal lobe the first thing that strikes one is its apparent increase in size owing to the throwing backward of the fissure of Rolando, and the convolutions and sulci present considerable abnormality, they having a definitely too vertical direction in the posterior part of this lobe, whilst in the anterior part there is a tendency for them to run in a curved direction from before, backward and downward instead of following the usual horizontal direction; so that in this lobe also there is a tendency for the convolutions to assume a radial direction though in a much less marked manner than in the posterior part of the brain. Immediately in front of the fissure of Rolando and parallel with it, is a deep and well-marked sulcus, which runs in a vertical direction from the margin of the hemisphere into the Sylvian fissure, being interrupted only in one spot by a short, partially sunken, bridging convolution, which probably represents the præ-central sulcus. In front of this again is another deep fissure running in a vertical direction, but with a little inclination from before backward, which, however, stops short before it reaches the upper margin of the hemisphere, whilst at its lower extremity it is separated from the Sylvian fissure by a well-marked fold of convolution. Immediately in front of this again is a deep vertical fissure running upward for about an inch from the Sylvian fissure, which appears to represent the sulcus diagonalis. The ascending ramus of the Sylvian fissure is about normal in length, but the anterior horizontal ramus of this fissure is both deeper and longer than usual. The other sulci of this lobe have a somewhat irregular course, but the tendency is for them to run from before backwards and downwards, in a curved direction as before remarked. The convolutions of this lobe are large and well-formed, and have of course a direction corresponding to that of the sulci. On the orbital surface a deep and well-marked fissure runs from before backward, throughout the whole length of this lobe parallel with the sulcus olfactorius, and separated from the tri-radiate sulcus, which is formed much in the usual fashion, by a well-formed con-

volution, running parallel with it. The convolutions and sulci on the inner aspect of this hemisphere have a quite typical arrangement.

The *Basal ganglia, Pons, and Medulla*, and *Cerebellum* shared in the general congestion, but appeared quite normal, except that, as indicated in the weights given above, the right half of the cerebellum was somewhat smaller than the left.

Microscopical Examination by Dr. Campbell.

Left Hemisphere (fresh cortex, stained with aniline). Sections taken from the para-central lobule showed a somewhat dense epicerebral fibrillation; in the first layer a few large succulent spider cells are visible, but the nerve-cells throughout the section show practically no morbid change. Vessels normal.

Crus, *Pons* and *Medulla* (method of Pal and ammonia-carmine). Sections made from the level of the third nucleus down to the olives, showed no sign of secondary degeneration or disease, except that it was thought that the superior cerebellar peduncular fibres were a trifle larger on the left side than the right.

Remarks.

Although it may be impossible to determine the exact date of the lesion in this case, it may, I think, with some confidence be assigned to the period of intra-uterine life, and probably to about the sixth or seventh month of development. Kundrat drew special attention to a radiate arrangement of the convolutions round the margins of the cavity, which he found in some of his cases, and considered that such an arrangement pointed to the congenital origin of the cases in which it was found, and on this point I am quite in agreement with him; the profound changes which have occurred in the grouping of the sulci and gyri in the case just described, point clearly to some morbid agency at work at a comparatively early period in the development of the brain. As to the nature of this agency, however, there seems room for more difference of opinion, and, indeed, writers on the subject have expressed very diverse views with reference to it. Into a general discussion of the question space will not

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permit me here to enter, but I would point out that it is incorrect to use the term "porencephaly" as connoting a separate pathological entity, since different cases undoubtedly have their origin in different pathological processes. In the case before us the chief part of the destroyed area, that, namely, comprised by the lower ends of the two central gyri, anterior part of supra-marginal gyrus, posterior convolutions of insula, and portion of posterior part of first temporo-sphenoidal gyrus, is just that region which is very liable to be destroyed by softening in adult life and in old age, as a result of obstruction of a branch of the middle cerebral artery by thrombosis or embolism; and that the greater part of the lesion in this case, as above indicated, has had such an origin is hence strongly suggested. But it seems impossible to account for the deep fissure which cuts across the temporo-sphenoidal lobe—that is to say for that portion of the fissure which lies below the first temporo-sphenoidal gyrus—on any theory of vascular obstruction, or localised encephalitis; and it seems probable that we have here to deal with a true developmental defect. Such an idea is favoured by the great depth of the fissure in proportion to its width, by the neatly rounded ends of the convolutions abutting on it showing no trace of previous erosion, and by the manner in which it runs without break into a deep normal fissure, the third temporo-sphenoidal.

That the main portion of the lesion which has produced the porencephalic cavity in this case has been instrumental in causing the profound changes in the grouping of the sulci and gyri around it hardly admits of doubt; and that such a disturbing influence on development operating on the temporo-sphenoidal lobe may in like manner have caused the fissure cavity which cuts across it, seems a reasonable supposition. On this view, then, the main portion of the porencephalic cavity (as above defined) would have had its origin in a definite pathological process; whilst a minor portion would be due to an abnormality of development resulting therefrom. Such a view would at any rate account for the appearances presented, without inventing imaginary arterial distributions to fit in with them.

Case 2.—John Y., aged 21 years, had been an inmate of Rainhill Asylum for five and a-half years. He was sent from the workhouse, and no friends ever came to see him, so that, unfortunately, it was impossible to obtain any history of the case. He was about 5 ft. 2 in. in height, and 98 lbs. in weight; poorly developed. His features were very coarse and heavy, and he had no growth of hair on his lips or chin. Ears large and coarse, directed forwards. He had a left-sided partial spastic hemiplegia; the left arm was retained in the position of flexion, pressed against the side, and showed but little voluntary movement, whilst the left leg was the subject of a marked talipes equino-varus; the patient could, however, walk fairly well without assistance; the left arm and leg were also much shortened, and exhibited both in the muscular and osseous systems a remarkable diminution in size in all measurements as compared with the opposite limbs. The knee-jerk was exaggerated on the left side. His viscera appeared sound. Mentally he was an idiot of a most degraded type; he had a most fatuous expression; he had no power of speech, and showed hardly the least sign of intelligence, scarcely seeming to understand the most simple things; he masturbated freely, and was wet and dirty in his habits; he had to be washed and dressed, and required attending to almost like an infant; he was, however, able to feed himself. He was subject to frequent severe epileptic fits, having on an average about 180 annually. He eventually died of phthisis in July, 1896.

Autopsy (*thirteen hours after death*).

Thorax.—Both lungs full of tubercle and contained cavities at apices.

Heart.—Normal.

Abdomen.—Abdominal viscera normal, with the exception that the ascending colon contained a few tubercular ulcers.

Skull.

Cranium.—Antero-posterior diameter, $6\frac{1}{2}$ inches; temporal diameter, $4\frac{1}{2}$ inches; considerable asymmetry of the skull cap, the bones of the vault on the right side being distinctly smaller than those on the left; the frontal, temporal, and occipital fossæ on the right side are also clearly smaller than those on the left; the crista galli is pushed towards the right side; the left cerebellar fossa is smaller than the right. The lines of the middle meningeal

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arteries are clear on both sides, but the ramifications are more distinct and deeper on the left. Sinuses contain black and soft white clot. Dura mater markedly relaxed on right side, not decidedly thickened anywhere; no unusual adhesion to bone, but a few more adhesions to arachnoid than usual on both sides. About 1½ oz. of clear fluid in subdural space, but no blood or membrane. The arachnoid does not dip down between the hemispheres in the frontal regions, and the two inner surfaces of the frontal segments are loosely adherent to one another. Arachnoid and pia covering the whole of the right hemisphere, well vascularised, markedly thickened, opaque, and œdematous, and not adherent to the subjacent cortex of those convolutions which remain intact (to be subsequently described), but strips quite easily. On the right side a small anterior cerebral artery can be made out, but one can only find traces of a middle and posterior cerebral; the right internal carotid is markedly smaller than left. On the left side the pia-arachnoid is practically normal.

Left Hemisphere.—The ascending parietal convolution at its lower end, for a distance of about 2 cm., is markedly shrunken so as to form a mere ridge of nervous tissue, the atrophied part, however, does not extend quite to the lower end of the gyrus, but is separated from the Sylvian fissure by a normal fold of convolution. There is, however, no other lesion, and the rest of the hemisphere may be said to be perfectly normal. The convolutions are large and well formed, and follow for the most part a typical course, the only exception being that there is an unusually deep external parieto-occipital fissure, which extends for about 3 cm. on to the external aspect of the hemisphere, and joins the posterior part of the intra-parietal sulcus.

The Right Hemisphere resembled at first sight a large cyst, and was indeed converted for the most part into a bag of fluid. Broadly speaking, the cerebral substance has almost entirely disappeared over the posterior two-thirds of the brain, the only portions of the brain remaining partially intact, being a part of the frontal lobe and the extreme tip of the occipital. The best preserved portions of the frontal lobe are the convolutions of the mesial aspect and of the orbital surface; on the mesial aspect indeed, the first frontal gyrus (marginal gyrus) and the gyrus fornicatus are well formed for about the anterior two-thirds of their length, although reduced to about one half the normal size, the latter gyrus, however, extending a little further back than the former. The posterior terminations of these two gyri are not rounded off, but are somewhat sharply and irregularly cut. The

continuation of the first frontal gyrus on the orbital surface—the gyrus rectus—is also well formed, as are also the orbital continuations of the second and third frontal gyri; in fact, the convolutions of the triangular orbital plate may be said to be intact, although like the gyri on the mesial aspect, reduced in size, but the outermost portion of this plate is turned upwards by the distortion of the brain, so as to appear on the exterior instead of the under surface of the brain. On the external aspect the ascending frontal gyrus has nearly disappeared, though remains of it can be made out; of the frontal gyri in front of this the first or superior is fairly well formed, though a portion of the anterior end has disappeared; the second frontal gyrus is fairly, though imperfectly, developed in its posterior half, whilst the anterior half has quite disappeared, the convolution again re-appearing on the orbital plate; the third frontal gyrus has nearly disappeared, the anterior two-thirds having gone altogether, and the posterior third being only represented by a few vermicular remains of gyri—the portion of the gyrus which bends round to assist in forming the orbital plate is, however, fairly intact. The convolutions of the island of Reil have completely disappeared, their former position being merely indicated by a few faintly elevated lines. At the posterior end of the hemisphere the convolutions of the extreme tip of the occipital lobe are fairly intact, although reduced in size. The whole of the brain, however, between this part and the frontal region, comprising the whole of the parietal lobe, the whole of the temporo-sphenoidal lobe, and the greater portion of the occipital lobe, may be said to have almost entirely disappeared, there being left throughout the whole of this large region merely an exceedingly thin layer of nervous tissue adherent to thickened membranes upon which outlines of former convolutions can here and there be faintly mapped out. In places, indeed, the nervous substance has gone altogether, nothing being left but thickened pia-arachnoid and ventricular ependyma, and where a layer of nervous tissue exists between these two membranes it is for the most part no thicker than writing paper, but as the frontal lobe is approached the remains of nervous tissue become more pronounced, and the region comprised by the two central gyri is less completely destroyed than the other parts of the area indicated. There is, however, no actual communication between the lateral ventricle and the surface of the hemisphere, and the cystic appearance presented when the brain was first removed was caused by the great distension of the lateral ventricle, and in particular of its posterior and descending

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cornua, the outer wall of the cyst being formed by thickened pia-arachnoid and ventricular ependyma containing in places remains of nervous tissue as described above. The ventricle contained about 3½ ozs. of clear fluid.

The *Corpus Callosum* is fairly well developed in its anterior half, although reduced to about one half the normal size, but it rapidly tapers off as the anterior region is receded from, and is represented in its posterior half merely by a very thin film, about the thickness of tissue paper. The *corpus striatum* and *optic thalamus* are reduced to about one half the usual size, but are otherwise normal in appearance and consistence. (Owing to the disappearance of the temporo-sphenoidal lobe, these bodies form a prominence in the anterior part of this region when the brain is viewed from the external aspect.) The *corpora quadrigemina* are symmetrical, but appear smaller than normal. The third ventricle, velum interpositum, infundibulum, pituitary gland, and pineal gland can be seen as usual.

Cerebellum.—The left half is appreciably smaller than the right, the diminution being general, and not confined to any particular region. The *crus* on the right side is very small as compared with the left. The *fornix* is very small. The *pons* and *medulla* are asymmetrical, showing a diminution in size on the right side as compared with the left. *Right olfactory nerve* and *bulb* appear normal. *Left optic nerve* remarkably reduced in size; right less so.

Weights.

Encephalon (with contained fluid) ...	820	grammes
,, (drained of fluid)	700	,,
Right hemisphere	102	,,
Left ,, (unstripped)	455	,,
,, ,, (stripped)	433	,,
Cerebellum...	118	,,
Pons and medulla...	25	,,

Microscopic Examination of Crus Cerebri, Pons Varolii, Medulla Oblongata, and Spinal Cord, &c., by Dr. A. W. Campbell.

Serial transverse sections of the crus, pons, and medulla were made, and every fifth section mounted and examined. From the spinal cord, sections from every alternate segment down to the conus terminalis were taken. The stains employed were—(1) the

method of Pal, for the demonstration of medullated nerve fibres, and (2) ammonia carmine, for the display of cellular and connective tissue elements.

Crus Cerebri (Plate III., fig. 1).—In all the sections of the crus marked asymmetry is evident, the right half being obviously smaller than the left; this asymmetry is due to non-development of a number of structures pertaining to the right side. The right pedal system is exceedingly small, possessing not more than one-third of the bulk of the left, and it is interesting to note that while its inner third is fairly rich in nerve fibres (probably the frontal pontine tract), its outer two-thirds is almost entirely deprived of them. The right brachium conjunctivum, or superior cerebellar peduncle, is likewise reduced in size; above the decussation, where the two tracts lie close together on either side of the middle line (forming the so-called "white nuclei"), the right is seen to be barely half the size of the left, and the decussation, which takes place lower down, is of course very unequal. The right lemniscus is much paler and more slender than the left; also the bundle from the lemniscus to the pes, which is normal on the left side, can scarcely be defined on the right. The posterior longitudinal bundles appear normal on both sides. The fibres streaming over the upper border of the red nuclei towards Meynert's decussation (Meynert's "*fontainartige Fasern*") are distinctly scanty on the right side, and Forel's decussation is ill-defined. The nuclei and roots of the third nerve are approximately equal on either side. The anterior corpora quadrigemina seem to be normal, but the right posterior corpus quadrigeminum is obviously smaller than the left; its myelinic network is not so complex, its nerve cells are less numerous, and its brachium, compared with that of the left, possesses hardly any healthy fibres. The right red nucleus is distinctly diminished in size; it shows an excess of connective tissue, and is poor in nerve fibres—in its lower two-thirds especially. The right substantia nigra Soemeringii is reduced in extent, but the myelinic plexus in this structure on the two sides is alike.

Pons Varolii (Plate III., fig. 2). The asymmetrical appearance of the sections is altered in character as one proceeds down the pons. Owing to the decussation of the fibres of the superior cerebellar peduncles, the right peduncle becomes the larger, while the left diminishes in size correspondingly; hence the right upper half of the section appears greater than the left, while in the lower half of the section the disparity remains the same as it was above. With regard to the course of the right pyramidal tract in the pons, only a few small bundles of medullated nerve fibres can be seen,

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and these gradually decrease in calibre, and eventually disappear at about the level of the sixth pair of cranial nerve nuclei. Below this the position of the right pyramidal field is just indicated by some contracted fields containing fibrous tissue. The pyramidal tract on the left side is normal. With reference to the superior cerebellar peduncles, that on the right side progressively gains in size over the left as the decussation proceeds, and finally when the crossing is completed it is to be noted that the left is little more than half the size of the right. The disparity is best seen where the two structures, having assumed their position on either side of the fourth ventricle prior to disappearing in the cerebellum, can be viewed in complete transverse section. The right lemniscus inspected in its varying positions along the pons continues markedly smaller than the left. The diminution in size seems to be more pronounced in the mesial than the lateral portion. The bundles of fibres composing the middle cerebellar peduncles seen in transverse sections, sweeping across the ventral segments of the pons are of somewhat smaller calibre on the left side than on the right. Also at the lower end of the pons the bundles of fibres proceeding to form the corpora restiformia, or inferior cerebellar peduncles, appear reduced in calibre on the left side. (This reduction in size of the left middle and inferior cerebellar peduncles is obviously the outcome of the crossed cerebellar atrophy consequent on the cerebral defect.) The posterior longitudinal bundles and the small bundles of fibres cut transversely in the upper part of the tegmentum appear of equal size, but the delicate horizontally directed bundles of the tegmentum are more obvious on the left side than on the right, though the right tegmentum forms a more extensive field than the left. The nuclei and roots of all cranial nerves in the pons may be considered healthy.

Medulla Oblongata (Plate III., fig. 3.).—There is not a single medullated nerve fibre in the right pyramid, its position is simply indicated by a small crescentic area which stains deeply with ammonia carmine and remains unstained by the method of Pal. The left pyramid is of normal bulk, but it is rounded instead of being flattened on its inner surface, evidently owing to withdrawal of the support normally given by the opposite pyramid. The nucleus of the right anterior cornu seen above the pyramidal decussation is distinctly small in size and poor in nerve cells, but below the decussation it rapidly gains a normal aspect, while the left anterior cornu, the nucleus of which was normal above, now diminishes in size. The reduction in volume of the right lemniscus is well seen

between the two great olives, and at the lemniscar decussation (the raphe), and below it, the fibræ arcuatæ internæ of the right side form much larger bundles and are much more prominent than the same fibres on the opposite side, also the nuclei of the funiculus gracilis and funiculus cuneatus, as well as the substantia gelatinosa Rolandi, to which the arculate fibres run are distinctly larger and contain more nerve cells on the right side. The hilum of the right great olive is wider than that of the left, and the right olive generally is expanded, but this is probably the result of the absence of the right pyramid, and of no special degenerative significance, because there are no changes in its nerve cells, or myelinic plexus of any import, and the accessory olives, with the exception of slight alterations in position, are equal on either side.

Spinal Cord (Plate IV., fig. 2).—Along the whole length of the spinal cord down even to the lower sacral segments there is most pronounced asymmetry, the left half being greatly smaller than the right. The inequality affects grey and white matter alike, and is appreciated best when the lateral columns are compared. The area of the left lateral pyramidal tract is marked by the presence of a small sclerosed field containing few healthy nerve fibres, and this sclerosis can be followed down as far as the eleventh dorsal level. In the position of the right direct pyramidal tract there is no sclerosis, but the anterior column on that side is evidently smaller than the left, and the disparity is maintained as low down as the eighth dorsal segment. The left anterior cornu is smaller, and contains less nerve cells than the right, all the way down the cord; the lateral projection at the cervical and lumbar enlargements has particularly suffered from wasting. The left posterior cornu is short and stout compared with the opposite one, but seems to contain as many nerve cells.

Peripheral Nerves.—The musculo-spiral and sciatic nerves of either side were examined; those of the left side presented a general reduction in calibre of the fasciculi, a thickening of the perineurium, a marked falling out of nerve fibres, and an undue prevalence of endoneural connective tissue and nuclear elements, while those of the right side were approximately healthy.

Microscopic Examination of Cerebral Cortex.—Portions of the cerebral cortex of the right hemisphere, taken (1) from the first frontal convolution, where it had maintained its shape, and (2) from the exceedingly wasted temporal segment, were closely examined for nerve cells and nerve fibres. In sections of the latter no sign of a nerve cell or a nerve fibre can be made out; all that is to be seen is a thick and cellular pia-arachnoid membrane,

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enclosing blood vessels with thickened walls, and beneath this a cellular layer, which seems to be the thickened ventricular ependyma. In the sections of the first frontal convolution are numerous nerve cells, and these can be differentiated into layers; the first layer is thin and pale, and appears to contain no nerve cells; the second layer shows only a few small pyramidal cells, with a large nucleus; the layer of large pyramidal cells is soon reached, and continues for some depth; the cells of this layer are of smaller size than normal, and their processes are stunted; they possess a large nucleus, a definite nucleolus, and the investing protoplasm, though thin, shows a definite arrangement of chromophilic particles. Before the white matter is reached there is a recognisable layer of oval or fusiform cells. All the cortical blood vessels are thickened. In the white matter are numerous nerve fibres, but the reticulum formed by them is far less dense than that seen in the normal brain.

Remarks.

It is perhaps a question whether this case should in strictness be included under the term "porencephaly," but similar cases have been described by Kundrat and others under this head, and it is convenient provisionally, at any rate, so to classify it.

In discussing the pathology of the case, it is clear, from the nature and distribution of the cerebral loss, that we have not here to deal with a case of developmental defect, but that the lesion has been caused by a definite destructive process of some sort. And the first thing that strikes one is that the lesion corresponds in a general way with arterial distribution, the portions of the hemisphere which have suffered most being those supplied by the posterior and middle cerebral arteries, whilst the region supplied by the anterior cerebral is comparatively intact; and the blocking of the two former vessels by thrombosis or embolism occurring during fœtal life or about the period of birth or early infancy, might be considered a sufficient explanation of the appearances presented. It may be remarked here that the small size of the opto-striate bodies, seems clearly to be due to want of development, their general integrity being incompatible with

the idea that their arterial supply has been cut off. If, therefore, the above-named vessels—the posterior and middle cerebral—have been occluded, they must each have been blocked independently just after the vessels which supply the basal ganglia, have been given off. But the distribution of the lesion is not so exactly confined to arterial territories as we should expect to find it, had the lesion the origin above suggested. For, in the first place, though the territory supplied by the anterior cerebral artery is that which is most intact, this region is still by no means free from invasion. For on the exterior aspect of the frontal lobe, it is the *anterior* portions of the first and second frontal gyri, which are most destroyed—the parts of these gyri, that is, which get their blood from the anterior cerebral; whilst on the mesial aspect the intact convolutions do not extend further back than about the middle line of the brain, whilst the portions between this and the parieto-occipital fissure—paracentral and quadrate lobules, &c.—have completely gone. But it is well known that the anterior cerebral artery supplies the whole of the upper edge and mesial aspect of the hemisphere, as far back as the parieto-occipital fissure, so that portions of the area supplied by the anterior cerebral have disappeared as completely as the region supplied by the other vessels. Again, at the other extremity of the hemisphere, it is difficult to account for the comparative retention of the tip of the occipital lobe upon the theory of vascular obstruction. That area, though small, is quite distinct, and must, therefore, have retained at any rate a portion of its blood supply from the posterior cerebral; but the remainder of the area supplied by this artery has so completely disappeared, that if the wasting were due to arterial obstruction, no partial occlusion of the artery would suffice to explain it, but the blocking must have been complete, and hence it is very difficult to see how any area, however small, can have survived.

A destructive encephalitis occurring during fœtal life or early infancy, would, no doubt, explain the appearances presented, although it might be difficult to understand how such a process could be started. I am rather, however,

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inclined to the idea that the lesion is the result of injury inflicted during the process of birth, not improbably by means of the forceps. A severe crushing of the hemisphere thus produced, with effusion of blood, might not improbably have caused such complete disorganization of the area involved, that recovery was impossible, and hence a slow process of absorption of the injured parts might have set in, which eventually produced the appearances found. The very slight lesion of the opposite hemisphere is quite compatible with this view.

This view, at any rate, harmonises with the facts that the lesion must have dated either from about birth or very early life, as evidenced by the asymmetry of the skull, and the arrest of development of the paralysed parts, and the non-development of the pyramidal tract: and it also fits in with the view that the patient was, so to speak, an accidental idiot—one in whom the idiocy was due, not to a congenital mal-development, but to some supervening accident (using this word in its widest sense); for the left hemisphere was, as I have elsewhere observed, well developed, and there is no reason to suppose, from an examination of this, that the patient would not have possessed the average intelligence of his class, if his other hemisphere had been in a similar condition. A history would, no doubt, have thrown some light on the case, but, as before remarked, no account was obtainable of the patient's antecedents.

NOTE ON LITERATURE.

Among the more important memoirs dealing with the subject of Porencephaly, the following may be mentioned:—

KUNDRAT. "Die Porencephalie, eine anatomische Studie." Graz, 1882.

AUDRY. "Les Porencephalies," *Revue de Médecine*. June and July, 1888.

SCHATTENBERG. "Ueber einen umfangreichen porencephalischen Defect des Gehirns bei einem Erwachsenen." *Beiträge zur path. Anat. und zur allg. Path.*, 5. Band, 1. Heft. Jena, 1889.

NORMAN AND FRASER. "A case of Porencephaly." *Journal of Mental Science*. Oct., 1894.

Anyone interested in the subject will find in the above communications abundant references to other published papers and cases, which limits of space will not permit of being reproduced here.

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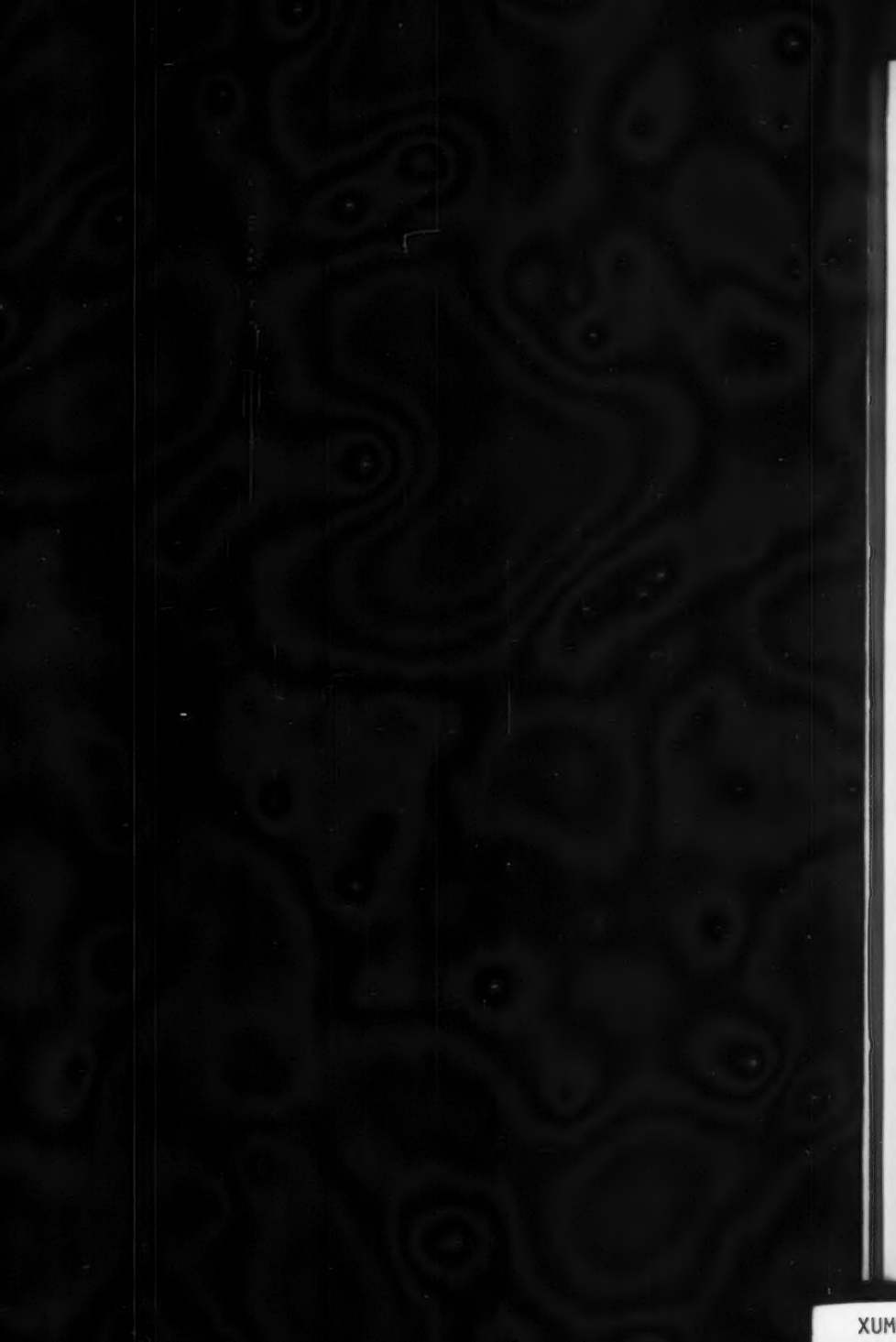

PLATE I.

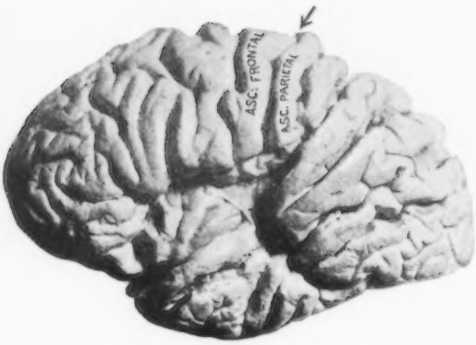

FIG. 1.

FIG. 2.

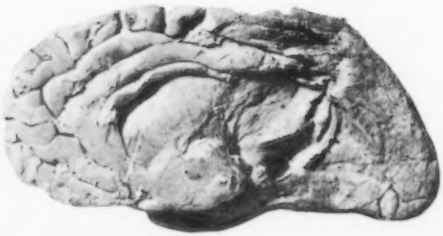

FIG. 3.

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PLATE II.

FIG. 1.

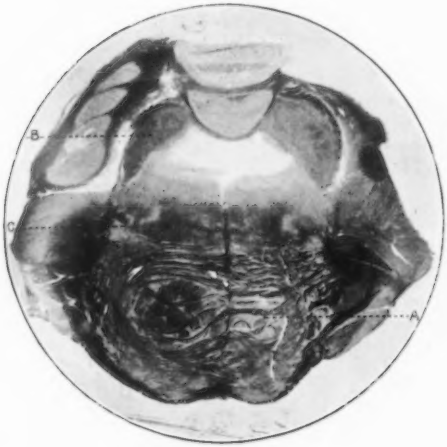

FIG. 2.

FIG. 3.

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PLATE III.

FIG. 1.

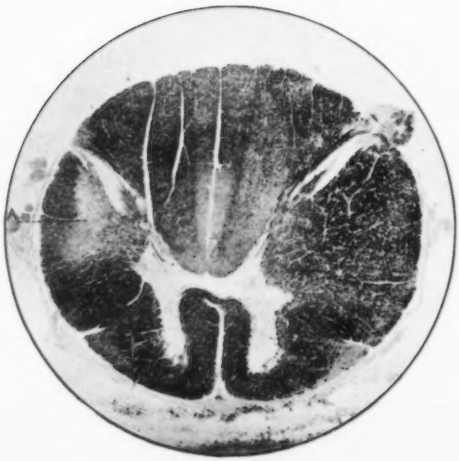

FIG. 2.

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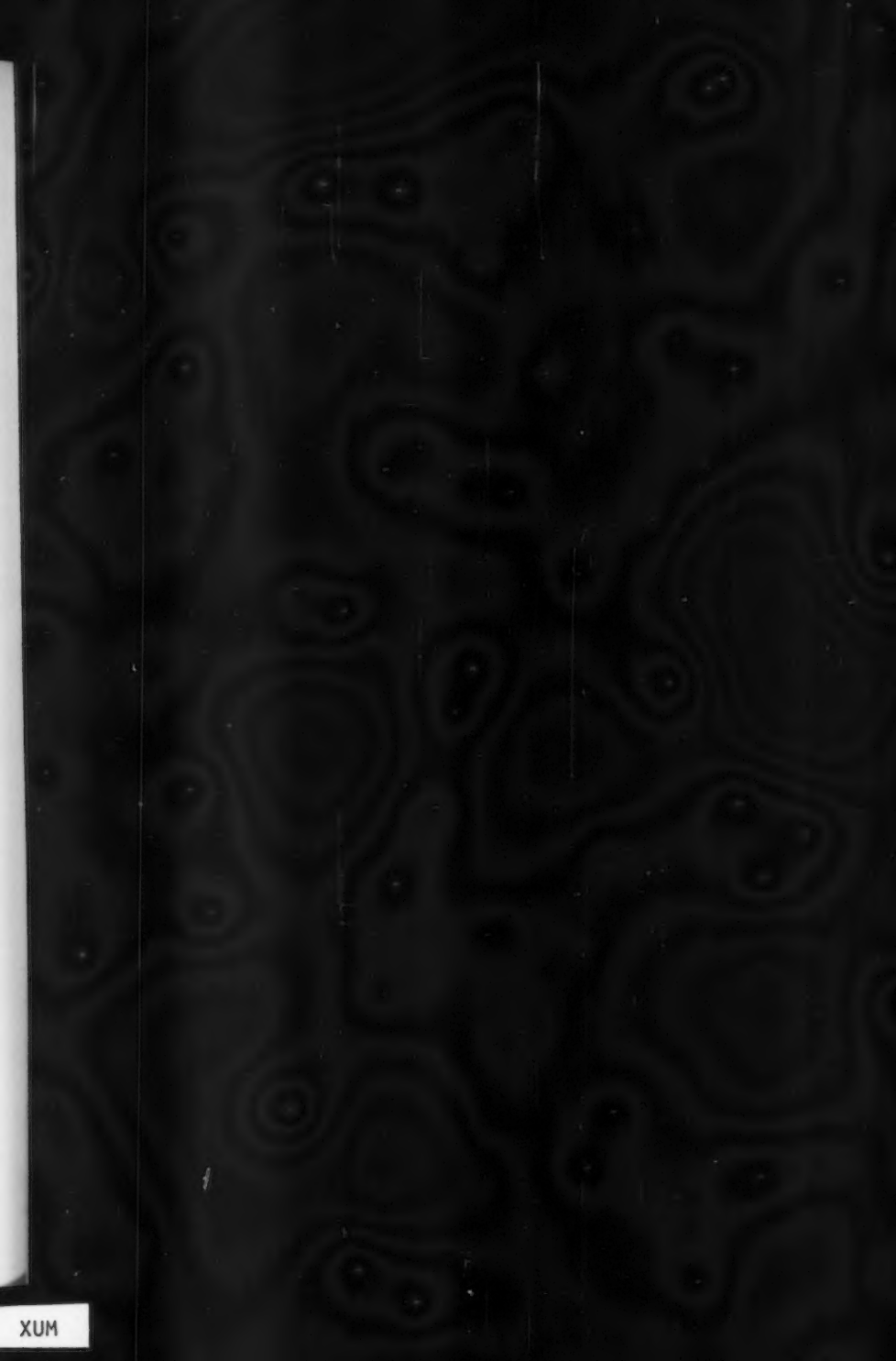
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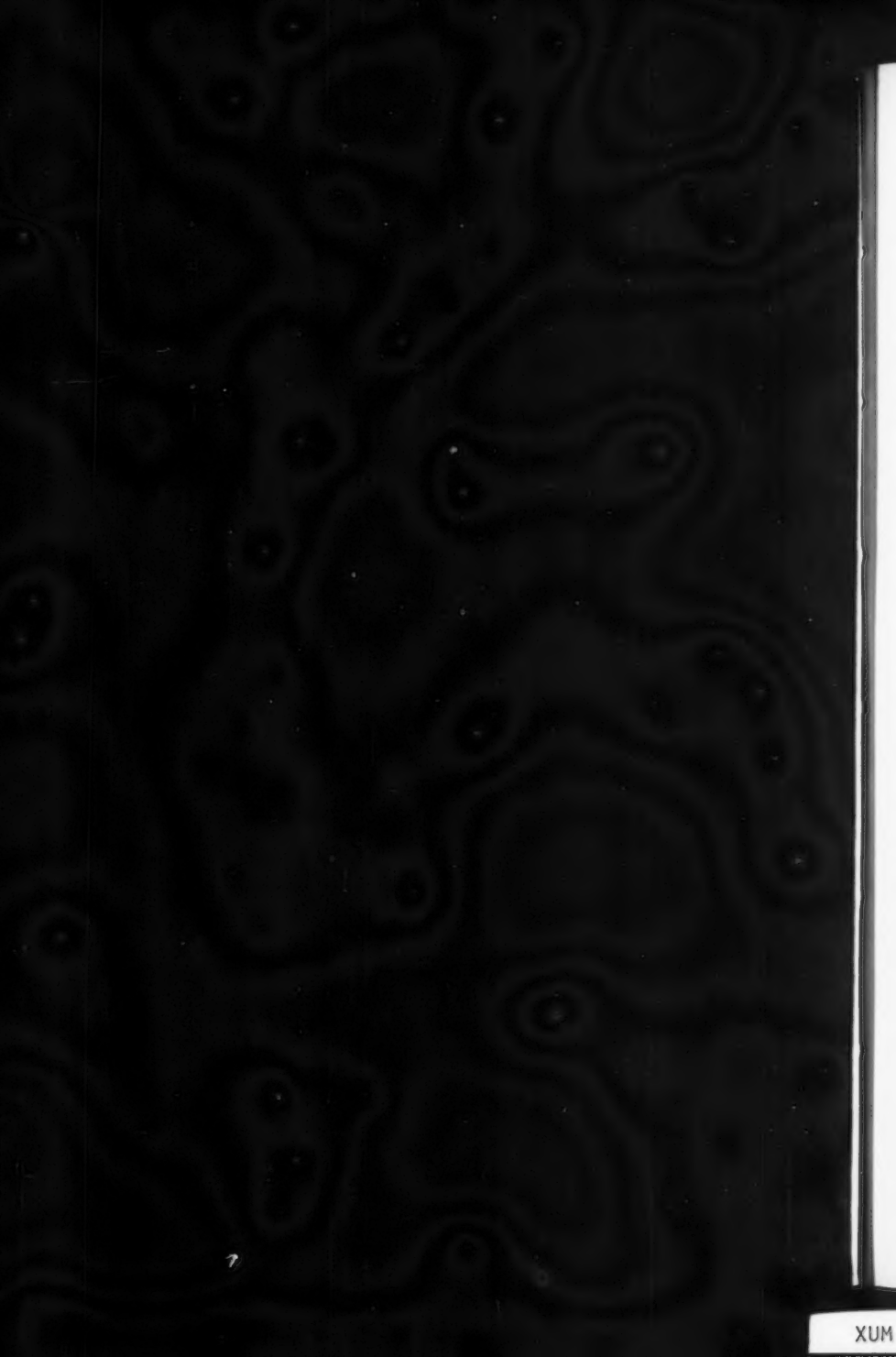
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EXPLANATION OF PLATES.

PLATE I.

Case 1.

FIG. 1.—Left hemisphere of J. B., outer surface. Absence of lower ends of two central gyri and anterior half of supra-marginal gyrus, &c., together with fissure cutting across temporo-sphenoidal lobe. Radiate arrangement of convolutions round the porencephalic defect. The arrow indicates the fissure of Rolando.

Case 2.

FIG. 2.—Right hemisphere of J. Y., outer surface. The prominence in the temporo-sphenoidal area consists of the opto-striate bodies, covered by the membranous remains of the temporo-sphenoidal lobe. Immediately in front of this projection is seen the orbital plate of the frontal lobe, drawn upwards and outwards, so as to appear on the outer surface of the hemisphere.

FIG. 3.—Right hemisphere of J. Y., inner aspect.

Case 2.—*J. Y.*

(Explanation of Plates II. and III.)

The figures are all reproductions of photo-micrographs of sections, stained by the method of Pal—medullated nerve fibres appearing black, nuclear matter and sclerosed tissues white.

A = Pedal system, or fibres of motor tract.
B = Brachium conjunctivum, or superior cerebellar peduncle.
C = Lemniscus, or fillet.
D = Substantia nigra Soemmeringii.

PLATE II.

FIG. 1.—Section through the crus, above the decussation of the brachia conjunctiva. The posterior corpora quadrigemina appear at the top of the figure, the pedal system at the base, and the brachia conjunctiva on either side of the middle line. There is pronounced asymmetry, owing to reduction in size of the right pedal system, substantia nigra, brachium conjunctivum, lemniscus, and posterior corpus quadrigeminum.

FIG. 2.—Section through upper part of pons, below the decussation of the brachia conjunctiva, showing the left brachium (B) to be much the smaller. Right motor tract (A) absent.

FIG. 3.—Section through upper end of medulla oblongata. Right pyramid (A) absent, and interolivary layer (lemniscus) markedly narrowed on right side. General asymmetry obvious.

PLATE III.

FIG. 1.—Section through lower end of medulla oblongata. Right pyramid (A) absent. Fibræ arcuatæ internæ (C) normal on right side, wasted on left. Nuclei of funiculus gracilis and funiculus cuneatus and the substantia gelatinosa Rolandi, of small size, on left side.

FIG. 2.—Section through spinal cord in upper cervical region. Left half greatly smaller than right. Sclerosis in position of left lateral pyramidal tract (A). Right anterior column reduced in size. Left anterior cornu smaller than right. Left posterior cornu short and stunted.

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ON A CASE OF PSYCHRO-ÆSTHESIA.

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I. F., bricklayer, aged 52, has been under my care at Regent's Park Hospital for Epilepsy and Paralysis since March, 1894. He complained then, and has constantly complained since, of a sensation of painful coldness chiefly affecting the right lower extremity at first, but more recently involving to a less extent the right arm and left leg.

He gives the following history:—

His general health has always been good, but he has always suffered from cold hands and feet.

In July, 1892, whilst kneeling at work, his right knee became locked, and always did so subsequently whenever he knelt down.

In August, 1893, he fell whilst walking from a similar cause. Effusion took place into the joint, which incapacitated him for work for several weeks. He attended St. Bartholomew's Hospital for this and for coldness in the foot and leg, and was found to have loose cartilage in the right knee. The loose cartilage under treatment soon ceased to give trouble, but the sensation of coldness in the leg has increased until it amounts to positive pain, which he thus describes:

"The coldness in my right leg has gradually got worse, with peculiar pains such as aching, smarting, and numbness, all at once and mixed together. If I put my foot to the ground the cold pain is there with aching and numbness, and it gets worse the longer it is on the ground. If I sit with it on the floor it is the same, and like a cold wind blowing on my thigh. After dressing I have to sit with my foot up in front of the fire, and then the cold wind seems on my thigh the same. I cannot walk for more than a few minutes without wanting to sit down and raise

my leg. When the coldness first came on it seemed as though my foot was placed on ice, in the hollow of the foot, but now it is all over the foot, top and bottom alike, and the coldest pain comes from the roots of my toes. The foot and leg always seems as though there was a lot of insects trying to pass each other and cannot, and so cause the pain such as I have described. I am easiest when I can put my foot and leg in a hot bath, or when lying in bed, but I cannot bear anything to touch me on the leg besides the bedclothes, as it seems to stop all circulation and cause numbness and aching."

Since he has been under my care, this account of his symptoms has not varied, except that he has recently complained of sensations of coldness in the right arm, particularly the shoulder, and in the left leg. The right leg is, however, much the more severely affected. He has been unable to work for two years, and has increasing difficulty in getting about.

Present Condition.—He is a strong-looking but loosely-built man. He complains of no symptoms but those mentioned. His thoracic and abdominal organs are sound. His digestion and appetite are good There is nothing abnormal to be seen about the upper extremities. The left lower extremity has some varicose veins. The right lower extremity is flabby. The thigh measures an inch less, and the calf three quarters of an inch less, in circumference than the corresponding portions of the opposite limb. There are no varicose veins and no œdema here. There is now no evidence of loose cartilage in the knee joint. The limb is weak in proportion to the wasting. He uses two sticks in walking for fear of falling, but is able to walk unassisted.

Reflexes.—The knee jerks are present and equal on both sides. He can stand steadily with eyes shut and heels together.

Sensation.—Tactile sensation is perfect, also appreciation of heat and cold, but the application of cold to the right leg is distressing, whilst that of heat is agreeable to him. There is no difference in the temperature of the two limbs. Both are warm to the touch, and he admits that this is so, although the sensation of coldness in the right leg obliges him to wear two or three thick stockings.

Pain.—He recognizes a pin's prick readily and winces. He states that he is unable to feel faradism applied to the right leg by means of a wire brush so readily as he used to do when under this treatment two years ago. He certainly tolerates, without showing signs of pain, a much stronger faradic current than I could endure myself. The electrical reactions are quite normal.

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His muscular sense is unaffected. He imitates correctly any attitude in which one leg may be placed, with the other, and describes their positions correctly, his eyes being closed. There are no tender points nor patches of anæsthesia in the course of the nerves of the limbs. His sensations of cold are not confined to the area of distribution of any particular nerves.

His family history is unimportant as regards nervous disorders.

Remarks.

Psychro-æsthesia (derived from the Greek Ψυχρός "cold") is the term used by Silvio (*La Riforma Medica*, February 17th and 18th, 1896) to describe symptoms somewhat resembling those of my patient, but differing from his in the limited area of their extent.

Silvio's case was that of a healthy man, aged 62, who for twenty years had suffered at intervals from a sensation of cold affecting the outer surface of the right thigh. The sensation was induced by standing, and relieved by lying down. He never had pain or anomalous heat sensations in the affected part. Ordinary sensibility seemed normal in the right thigh and elsewhere. Silvio contrasts this condition with that of subjective sensations of heat, which he terms "kauma-æsthesia."

As an instance of the latter, he relates the case of a man, aged 44, who for three years had complained of a burning sensation in the right thigh, at first confined to a limited area, but later involving almost the whole of the thigh, and accompanied by a feeling of deadness in the skin. It occurred intermittently, chiefly in the winter, was relieved by friction or by resting the body on the other leg, or by sitting down. Sensibility to pain and heat was normal, tactile sensibility was slightly weakened. Twenty years previously he had suffered for about a year from a sensation as of drops of water trickling down the inner surface of the right thigh.

Under the title "Meralgia Paræsthetica," cases similar to this last one have been described by W. K. Roth, of Moscow (Berlin, 1895). The painful sensations affected

chiefly the outer side of the thigh, with subjective or objective anæsthesia. The pain was mostly burning in character, and was rendered worse by standing or walking. The paræsthesia and anæsthesia were generally limited to the distribution of the external cutaneous nerve of the thigh, but the sensation of burning sometimes extended beyond this area. Roth regarded the symptoms as due to an affection of the above-named nerve, as their duration (in one case fourteen years) seemed to preclude the idea that they were dependent on an early lesion of the spinal cord.

Cases of paræsthetic meralgia have also been collected by Bernard, of Berlin (*Révue Neurologique de Paris*, Nos. 20 and 23, 1895), and by Escat.

More recently the subject has been discussed in the *New York Journal of Mental and Nervous Disease* (March and April, 1896) by Osler, Hirsch, and Weir Mitchell. Weir Mitchell mentioned patients in whom paræsthesia and pain in the thigh have turned out to be due to unsuspected periosteal disease.

Hirsch had met with a sailor, aged 50, who, after being shipwrecked and suffering from long exposure, as well as having to bear considerable weight on one leg for over an hour, was attacked by a peculiar sensation which he described as "burning cold," over a region the size of the palm of the hand, four inches above the knee, on the outer side of the thigh. He had suffered from syphilis eighteen years previously, and had been addicted to alcohol. Hirsch regards these as the predisposing, and the exposure as the exciting cause of the complaint.

The general opinion on these cases, to which my own bears a certain resemblance, appears to be that they are indications of local mischief. As such, they seem to be akin to those originally described by Weir Mitchell under the name of causalgia ("Injuries of Nerves," p. 159). But in Weir Mitchell's causalgia there is a history of direct injury, usually by gunshot, to the nerves themselves. The peculiar pain was associated with redness or glossiness of the skin, and with thickening of the nerves' trunks above the site of pain.

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Similar trophic changes are found in the nerves when causalgia is dependent on central lesions of the brain or cord. In my own case, however, and also apparently in recorded cases of meralgia, there is no evidence of such trophic degeneration of nerves. Causalgia again expresses the type of suffering in Morton's disease (metatarsalgia), and in the similar affection which I have described as "painful toe" (*Lancet*, March 19th, 1892). But in both these complaints a definite local cause exists in the shape of tight boots, which produce displacement of some of the metatarsal or phalangeal bones, which, accordingly, press painfully on the subjacent nerves. In my own case of psychro-æsthesia, and apparently in Silvio's, no such local cause can be discovered. The sensations in my case are not relieved by removal of the boot. Moreover, such an explanation would not account for the extension of the symptoms to the right arm and left leg.

There is no evidence of peripheral neuritis. As to other local causes, I at first thought that the vasor-motor apparatus might be at fault, as in the minor forms of Raynaud's disease; but in Raynaud's disease the numbness, tingling, and coldness complained of, are associated with actual loss of temperature and pallor of the parts affected; whereas my patient has no loss of temperature, local or general, no blanching, and no œdema of the limb. The only physical signs present are slight wasting and slight weakening of the right lower limb. It might be thought that these conditions are dependent upon the loose cartilage from which he has suffered. Yet neither this nor any other local cause can account for the involvement of the right arm and left leg.

It is, therefore, probable that the wasting and weakness, and, perhaps, the loose cartilage also, are indications of general dystrophy resulting from some central nervous conditions, which also affects the right arm and left leg similiarly, but to a less extent than the right lower limb. If central, the lesion may be either cerebral or spinal. If cerebral, one must first ask whether the symptoms are due to a mere psychosis. It must be admitted that his symptoms are mainly subjective. The only objective evi-

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dence of hindered sensation is his remarkable tolerance of strong faradic currents, the significance of which is obscure. The pathology of subjective sensations must always be elusive. One is apt to assume that it is non-existent in the absence of physical signs, and to attribute complaints, for which there is little to show, to malingering, hypochondriasis, or hysteria. The patient has nothing to gain by malingering, as he has enough money to live upon, at all events, for the present. His symptoms are not such as would be hit upon by a malingerer. He seems as much troubled by his presumably enforced idleness as by the sensations of which he complains. Hypochondriacs often discover strange symptoms, but they soon appear to tire of them, and invent new sets as the novelty of the old wears off. But this patient's account of himself has never varied during the two years I have known him. He is not, moreover, gloomy or depressed, but takes a semi-humorous view of his condition; being cheerfully apologetic when remedies fail, and also appearing grateful for any attempts to relieve him. This is unlike the ordinary hypochondriac.

The duration and unchanging nature of the symptoms, and the absence of special signs of hysteria, and of the hysterical temperament generally, are against the diagnosis of this affection. It is difficult, therefore, to dismiss the symptoms as imaginary, functional, or feigned.

As regards gross cerebral lesions; only multiple and bilateral affections could involve the sensory tracts of both legs and one arm; and it is extremely improbable that the face, the special sense tract, and also the motor tract, would escape were this the case. There is, moreover, no evidence, as far as I am aware, that the peculiar sensations, of which this patient complains, are ever associated with cerebral disease, either situated in the cortex, or elsewhere. On the other hand, there is abundant evidence that parallel, if not absolutely identical, sensations may be associated with disease of the spinal cord or of its nerve roots. This was well brought out in the discussion on "Dissociated Sensations as a Means of Diagnosis," reported in

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the *Journal of Nervous and Mental Disease*, New York, for April, 1896.

Perversion of sensations of heat, cold, pain, and of tactile sensibility, as well as of the muscular sense, may occur in spinal meningitis, whether due to curvature, fracture, or syphilis, and depends on involvement of the spinal nerve roots. Similar perversions of sensations may be due to disease of the spinal cord itself.

Dana, in the above-named discussion, summarised the effects of lesions in various parts in producing differentation of cutaneous sensations, as follows:—These effects were least, he said, in peripheral nerve lesions, increased in spinal root lesions, and again in central spinal lesions, most striking and complete in ponto-bulbar lesions. They became very slight again in cortical lesions.

In the present case, we may exclude lesions of the spinal roots, because they could not produce the wide-spread sensation of cold alone without giving rise to other palpable defects. We have excluded peripheral nerve lesions, and also cerebral affections. There only remains to be considered a lesion of the spinal cord.

His symptoms are not distinctive of disease of the lateral or posterior columns, but it may be that the anterior horns are implicated because of the slight wasting and weakness of the right lower extremity which prevail. The course of thermal sensations in the cord, and also the course of sensations of cold, are unknown. The frequency, however, with which such sensations are altered or annulled, together with those of pain, suggests that the paths of heat, cold and pain may be contiguous.

There is evidence that painful sensations pass through the posterior commissure on their way to the antero-lateral tract on the opposite side.

The disease in which all these sensations are frequently perverted or absent is syringomyelia; and it is possible that involvement of the posterior commissure in this disease may account for the symptoms.

According to Gowers, subjective sensations of heat and cold not unfrequently precede the characteristic symptoms

of syringomyelia. The duration of the symptoms here does not exclude the possibility that they are caused by this disease; for symptoms may be absent altogether, although the conditions of syringomyelia must have existed for many years.

It is usual in cases of syringomyelia for the upper extremities to be affected earlier and more seriously than the lower, doubtless because the site of the disease is usually in the upper part of the cord. But cases have been reported in which the converse order of symptoms prevailed. This may be a case in point; and, accordingly, we may have to deal with syringomyelia, in which the function of the posterior commissure is perverted though not destroyed. If so, the lesion must have commenced low down in the cord and spread upwards; whilst in order to account for the weakness and wasting of the right leg it must be assumed that the anterior horn is involved.

Treatment.— Silvio found that massage and thermo-mineral baths appeared to give relief in his case, but did not cure. In my own case, rest and the actual application of heat give temporary relief. Stimulating embrocations, ointments containing capsicum, and flying blisters applied to the thigh, have been beneficial for a time; electricity, in the form of galvanism and faradism, has been useless. All kinds of nerve sedatives and tonics have been tried in vain.

If the pathology of the case is, as has been suggested, any form of treatment can only be palliative.

NOTE.—The patient, who provides the subject of this paper, was shown at a meeting of the Harveian Society of London, on May 21, 1896. Of the speakers who discussed the case, Dr. T. Savill was inclined to agree that the symptoms were due to syringomyelia. Dr. Cagney suggested that some obscure lesion of the cerebral cortex might be the cause.

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STUDIES ON THE NEUROGLIA.[1]

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No so-called interstitial tissue has given rise to so much discussion as the neuroglia. The most far-reaching advances in our knowledge of it are, undoubtedly, the discovery of its fibro-cellular structure, and the recognition of its epiblastic origin. Upon this latter point, as far as it concerns pathology, we would here—for want of a better place—say a few words; and not for reason of any novelty, but because it does not as yet appear to enjoy general recognition. We refer to the classification of the gliomata, and to some incongruity of nomenclature still in vogue. If neuroglia is an epiblastic structure, then it follows that tumours composed of it are epiblastic also, and should not be classed under the mesoblastic growths, as some recent text-books[2] still do. Further, it follows that the term "glio-sarcoma," if it has any legitimate meaning at all, is a misnomer. This term is, at present, applied to tumours composed of cells which are supposed to possess characters common to glioma and sarcoma. Such a use of the word is, of course, radically wrong; and should only be employed to denote a simultaneous growth of epiblastic and mesoblastic elements side by side in a neoplasm—an occurrence, the possibility of which cannot be denied, however improbable.

[1] Being, in part, the substance of a paper read at the Annual Meeting of the British Medical Association at Carlisle, July, 1896.

[2] *E.g.*, Hamilton's "Text-book of Pathology," part i., p. 443.

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The rapid advance of our knowledge of the subject during the last decade we owe to Golgi's method. Applied to the neuroglia, the silver impregnation method has exhibited a wealth of fibres and cells where nerve-elements were once supposed mainly to exist; and has proved the neuroglial nature of structures, such as the septa of the spinal cord, which were once believed to be true connective tissue. But it is not our present object to give in detail the results obtained by this stain: they can now be found in every recent text-book of anatomy. It will suffice if we briefly enumerate some of the more important points as they concern us here. According to Golgi, Cajal, Kölliker, and the host of other observers, the neuroglia is said to consist of cells possessing processes varying in number, length, and thickness; and while the processes of some cells are smooth and undivided, others are rough, bushy, and branched. Transitional forms have also been noted. It would appear that the ependyma-cells lining the central canal are the parent stock from which the glia cells are primarily descended. In the amphioxus these central ependyma-cells compose the sole supporting structure; in the ascending scale of the vertebrata, cells can be seen which betray, to a greater or less extent, the original idea embodied in the ependyma-cell—that is to say, the pyriform body and the strong main process passing towards the periphery, actual or virtual. By a virtual periphery we mean, for example, the blood vessels, which, entering the nervous organ, invaginate the periphery, and with it the neuroglia. Additional prolongations, however, put in their appearance; and, in the most advanced form, the original main fibre has dwindled and decayed, allowing the secondary branches to constitute the principal feature of the adult neuroglia cell. This process of evolution, as exhibited in the various orders and genera of vertebrated animals, is passed through with leaps and bounds, and, probably, with short cuts, in the development of the human nervous system. Golgi's method, with its various modifications, has been the sole means by which these conditions have been studied in the lower animals, and,

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till recently, in the human being also. But before the close of last year another method saw publication. We refer to Weigert's neuroglia stain, a detailed account of which appeared in November last.[1] It is an intricate, and still somewhat uncertain, method. Our failures with it have been many—partly for the reason given; partly because suitable material is scanty, as a longer period than twelve hours after death lessens considerably the chance of obtaining even a fairly satisfactory result, if it does not ruin it altogether. Some parts of the nervous system, too, seem to resist the stain more than others, *e.g.*, the cerebral cortex; while some pathological conditions—above all, œdema—would appear to render satisfactory results particularly difficult to obtain. Our researches with this method are, therefore, still somewhat fragmentary; and it is our intention to give in this paper not any new results of normal anatomy, but to consider various points that have suggested themselves to us in the study of the pathological brain and spinal cord.

The effect of the method in the case of man—for to the adult human brain alone is it applicable—is briefly this: it dyes neuroglia fibres a fine blue; similarly all nuclei; it does not, however, stain fibrous tissue;[2] while the various nervous structures either remain unstained, or receive a delicate yellow tint. The advantages to the pathologist are that it is less capricious than Golgi's method, and that it permits the study of the distribution of the fully-formed neuroglia. Sections treated by this method present appearances which tend greatly to modify existing views. Hitherto we have believed, with Golgi, that in the human adult the neuroglia is purely a cellular structure; that its multitudinous fibres are not fibres in the strictest sense of the term, but cell processes. Weigert, however, on the strength of his method, comes to the conclusion that things are not so simple. He finds that the blue fibres shown in the preparations are no longer cell processes;

[1] C. Weigert, "Beiträge zur Kenntniss der normalen menschlichen Neuroglia," November, 1895, Frankfort-on-Main: M. Diesterweg.

[2] If one stage be omitted.

nowhere can fibres be seen joining the cell protoplasm; everywhere they pass over, under, and around the nucleus. He, therefore, concludes that here, as in the true connective tissue, the cell processes have become differentiated from the cell—have become separated from it. These, and only these fibres, are stained by the new method in the healthy brain or cord. But it would be an error to suppose that the human neuroglia can exist in no other form. For we reason that, as in some lower vertebrate, glia cells persist in several shapes characteristic of various stages of their evolution, so may neuroglia elements in man stop short of this terminal condition, in which fibre and cell cease to be a corporate whole. This intersection of fibres round the nucleus has still a superficial resemblance to a spider-cell; and if we adopt the term "astrocyte," as a synonym for "spider-cell," we may call these imitation spider-cells "pseudo-astrocytes."

This view of Weigert's has met with some opposition on technical grounds, which need not, however, concern us here; ample support can, on the other hand, be obtained from certain appearances to be noted under some pathological conditions, as, for instance, in the cerebral cortex of a general paralytic. What the most careful examination of healthy brain tissue fails to find—viz., a fibre stained blue like the rest, but still an integral part of the cell body—can frequently be seen in these morbid conditions. We see a number of fibres passing towards the body of one of these spider-cells; but as they approach it their tint gradually fades, merging gently into that of the cell protoplasm. All stages of this process can be observed—from that in which the fibre has taken up the faintest possible blue colouration to that in which it is difficult to tell whether the fibre is still part of the cell or not. It seems to have escaped Weigert's opponents that while the new statement of things runs counter to the simplicity of Golgi's theory, on the one hand, it tends to clear a difficulty on the other. We refer to the question of the multiplication of neuroglia cells. It had for some time been suspected that sclerosis—for instance, in the tracts of the cord—was

due to a proliferation of the supporting neuroglia; Weigert himself had demonstrated, by means of a method of his own, the presence of karyokinetic figures in such cases; and the difficulty naturally encountered was this: what becomes of the long and numerous branches of these cells in the process of division? No satisfactory explanation has ever, we believe, been offered on this point; refuge has usually been taken in assuming the existence of so-called abortive neuroglia cells—masses of protoplasm that have lain dormant from an early period of intra-uterine life. The new interpretation simplifies matters somewhat; for there can now be no difficulty in picturing the proliferation of cells that have previously parted with their processes. Weigert's stain, moreover, sets any doubts that may still have existed as to the glial nature of all sclerotic and cicatricial formations within the central nervous system at rest. Disseminated sclerosis, all so-called system diseases, and all reparative processes consist essentially of newly-formed neuroglia. If such a lesion be examined—and, for the sake of simplicity, let us take a sclerosis of the lateral or posterior columns of the cord—two changes in particular obtrude themselves. We find, on transverse section, that the neuroglia fibres are also cut transversely, appearing as fine blue dots between the nerve tubules; in other words, neuroglia fibre and nerve fibre run practically parallel to each other, the former having developed in the direction of least resistance. This is one feature; the other is, that the septa and trabeculæ are thickened, and that the perivascular neuroglia is denser and more plentiful than normal. In advanced chronic cases, not only do the septa become more bulky, but the subpial layer of neuroglia also, from which they take their origin, increases in thickness, even if a healthy tract intervenes. The crossed pyramidal tract, for example, may degenerate and sclerose, and the corresponding stretch of subpial neuroglia thicken; while the direct cerebellar tract between them remains apparently healthy. The most plausible explanation of these facts that we could think of is the following, based upon the principle that the proliferated cells, upon which a reparative process depends, tend to conform in shape to early types.

The type, in the present instance, is that of the modified ependyma cell, the chief characteristic of which is a main process, thick and strong, stretching to the actual or virtual periphery, and attaching itself there. A similar tendency is manifested by these newly-formed glia cells. Among their fibres one, or perhaps two, can be seen, coarser and thicker than the rest, passing to the periphery, which is usually represented by a blood vessel. Some of these branches are undoubtedly short, but their true length cannot in all instances be determined, for their course is tortuous, rarely continuing for any distance in the plane of section. It is these cells, with their strong "vascular processes," that have been dubbed "scavenger cells" by Bevan Lewis. In accordance with this explanation of ours is the interesting fact that the nuclei (or cells) within the subpial layer of neuroglia are altogether disproportionately few in number compared with the wealth of fibre; the natural inference being that the parent cells of most of these fibres lie elsewhere.

But the possibilities regarding the mechanism of a sclerosis are not yet exhausted. Weigert's new method does not, as we have already said, pretend to stain every fibre; there are regions in which we should, *a priori*, expect to find neuroglia, and in which a true sclerosis is known not unfrequently to occur. Such an area is the deepest layer of the cerebral cortex; and while scarcely a single blue fibre can be found in it in health, yet, under certain morbid conditions, a felt-work of neuroglia becomes visible. Fresh sections, stained with aniline blue-black, reveal, during the earlier stages of disease in this layer, a comparative richness in "scavenger cells," when contrasted with the poverty of the more superficial strata. A process of immigration from the upper to the deeper layers is, therefore, improbable; the scavenger cells, and the fibres derived from them, must have developed *in loco*, and from cells which had not attained to the final stage of fibrillation. Thus the old question again arises—if richly-branched astrocytes, or spider-cells, divide and multiply, what becomes of their processes? Investigations on this point

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are still a desideratum; we can only offer an opinion—and we do so with some diffidence—to the effect that, prior to their division, the astrocytes may possibly become differentiated into free fibres and free cell body. It is from comparative pathology, we think, that light may be expected. The normal anatomy of the purely nervous structures is already much indebted to this branch of science; but it is a remarkable fact that we know nothing, or next to nothing, of the effects produced upon the neuroglia by interference with the nervous centres of the lower vertebrates. What goes on, for instance, during reparative processes following an ordinary lesion; or how does the glia behave when the development of some particular system—say the central optic ganglia and tract—is experimentally arrested? We are in possession of numerous sections from the brain of a cat, which had been subjected to enucleation of one eye soon after birth; in none of them can any excessive development of true fibrous tissue be discovered. But in dogs, rabbits, and the like, the conditions are too complicated; we must descend still lower, and make animals in which the glia elements still bear a close resemblance to their ancestral cells—such as the batrachia, fishes, and, perhaps, birds—the subjects of our investigation. A trustworthy method, however, is still wanted for such a study, as Weigert's stain is as yet unavailable for comparative pathology. So far, then, our knowledge of the morbid anatomy of the neuroglia is confined to this fully developed sclerotic tissue.

We recognise that the process of fibrillation is not a confused, hap-hazard one, but follows certain laws; the felt-work formed is not so intricate, but that one or two fundamental principles cannot be traced. One is that the fibres develop in the direction of least resistance; and another that, to a certain extent, the newly-formed neuroglia follows in the path of the old. Examples of this are seen in the thickening of the subpial zone, and in perivascular sclerosis; but the pictures obtained in cases of superficial cerebellar sclerosis are even more instructive. In the outermost, or molecular, layer of the cerebellar

cortex, the neuroglia is normally represented by somewhat sparsely scattered fibres, passing radially towards the pia, while their cells of origin lie at the level of Purkinje's cells. With a slight and superficial lesion, this radial distribution becomes exaggerated, the fibres are crowded closer together, and appear decidedly thicker; but the one direction —outwards towards the pia mater, is strictly maintained. But given a severer case, then a further bundle of fibres makes its appearance at right angles to the last, running parallel to the surface of the lamina. Where these originate we are not prepared to say; but their direction is evidently that of certain axis-cylinder processes, derived from the small nerve cells of the molecular layer.

We would here like to call attention to an important point that has already been hinted at in some of the foregoing remarks: it is this: that an increase of neuroglia over a certain area is not necessarily tantamount to a primary nerve lesion of equal extent. As an example of this we would again instance the thickening of the trabeculæ passing through the direct cerebellar tract in some cases of lateral sclerosis; or, better still, to the cerebellar cortex, in which the radial fibres of the molecular layer undergo an increase throughout the whole thickness of that layer, even if the lesion is superficial. This is based on the principle that sclerosis following a given primary nerve lesion is due more to the activity of those neuroglia cells from which the pre-existing glia fibres of the affected area had originated, than to an influx of wandering cells—if, indeed, such a thing exists. The more complex the ramifications of axis-cylinders and protoplasmic processes in a given region, the more intricate and tangled is the neuroglial felt-work; it is from the simple anatomical conditions only that we can hope to unravel the principles of its development.

Defective as is our knowledge of the normal and pathological anatomy, that of the functions of the neuroglia is even more so. It is usually described as a supporting structure; and there is nothing to oppose this view. In one particular instance, indeed, as Weigert and Andriezen have pointed out, this function appears almost obvious—

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namely, in the case of the blood vessels. These are notoriously poor in adventitia, and the glia fibres passing obliquely along, and also across them, are well calculated to prevent "undue expansion," as Andriezen puts it. But it would seem to us that they may exceed this function under some conditions of perivascular sclerosis, and make the vessels too rigid, thus impeding the influx of blood to a greater or lesser area. The effects of such defective nutrition would be greatest where the arteries are terminal ones, as in the cerebral cortex; and this may perhaps be *one* reason for the merciless, ever-spreading course of some chronic diseases.

That such a wonderfully complex system should subserve no other function but that of a supporting structure, gives rise to a feeling akin to disappointment. The numerous theories on the subject are evidence of this; but, it must be confessed, we have got little further than theory.

In its first beginnings the glia possibly directs the outward passage, and, to some extent, the grouping of the neuroblasts. But from that period till near the end of intrauterine life, its function, if other than supporting, is a mystery; perhaps it lies latent, preparing for its subsequent duties. After birth it would appear to assume, to some extent, the *rôle* of an insulator. This is the opinion put forward by Sala y Pons and others, who based it on the apparently richer development of neuroglia in the white matter than in the grey. Weigert has demonstrated that Golgi's stain, with which these men worked, is anything but a sure guide in questions of quantitative distribution; and that, as a matter of fact, the grey matter is the richer of the two. Yet Sala's theory can stand. What necessity is there for such an insulator in the white substance where the medullary sheaths already serve that purpose? But in the grey matter things are different; here the axis-cylinders, and their collateral branches, break up into a multitude of naked terminal ramifications, interlacing among each other, and around the nerve cells. Here an insulator is obviously needed; and the neuroglia may well serve this end. Hence, Sala y Pons is probably right in his conclusion, though his premises are faulty.

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As a matter of curiosity, we may mention a theory, propounded by Rohde, for the invertebrates: that neuroglia fibres enter the nerve cells and supply them with nutriment. Such a condition has never, as far as we know, been observed in the vertebrata; and much might be said against it.

Diametrically opposed to it, however, is a theory, which appears to enjoy some popularity, and to which some reference is due: that is the "scavenger cell" theory of Bevan Lewis. According to this view, the glia cells possess not only the function of removing detritus, but also the power of attacking diseased nerve fibres and cells. In spite of the somewhat tempting nature of this theory, we cannot but think that shape and name of the cells in question have had their share in suggesting it. It is a theory as easily advanced as it is difficult to prove or disprove. No one, we think, has ever seen a spider cell in such relation to a nerve cell as to prove conclusively that it was attacking the latter. But to argue the function of a cell from its shape savours of the antique, and cannot be taken seriously. The doubts and uncertainties that still surround the lymphatics of the nervous system; the constant proliferation of neuroglia cells in all destructive lesions of that system; and the possession by these cells of a thick process attaching itself to a vessel wall, are the only grounds on which the theory can hope to stand. After all, it is simply a question of interpretation, and of the greater or lesser probability of one view over another. Twenty years ago Weigert developed the principle that diminished mutual resistance between tissues is followed by a process of proliferation. And the neuroglia is no exception; loss of power of resistance on the part of the nervous structures is followed by a corresponding increase of the neuroglia. Obliteration of the central canal furnishes an instructive example; the cubical cells becoming loosened, and packing the lumen of the canal, are followed by an increase and invasion of neuroglia—fibres from the latter passing between them in all directions. Yet these cubical cells are not attacked by spider cells, though such "scavengers" must

precede the stage of fibrillation. It may further be shown that a dead nerve cell is not necessarily removed, in spite of the presence of large numbers of spider cells; it may calcify, as is so often seen in sclerosis of the hippocampal gyrus in epilepsy. The distribution of the neuroglia is also a point of some moment; for there exists no parallelism between the amount of nerve tissue and neuroglia: witness the wealth of the latter in the anterior horns of the spinal cord, and its comparative scarcity in the motor cortex. In some of the lower animals, such as the calf, scavenger cells, distinguishable in no way from those seen in man under pathological conditions, are found in some abundance in the cerebral cortex. The deduction has been drawn that domestication has a deleterious influence on the nervous system, causing degeneration of its elements, though other evidence of such degeneration is wanting—a good example of what a too dogged adherence to a theory, in the face of facts, can lead to. Our own belief is that the so-called "scavenger cell" is but a form characterising the neuroglia cell in one period of its life-history; and that, in any proliferative process, this earlier stage must, in the natural course of things, be returned to, before fibrillation, as the final result, can be attained. It is true we have no new theory to offer; but if we cannot advance anything ourselves, we may, at least, sweep the path a little for what is yet to come.

NOTES ON GRANULES.

BY ALEX HILL, M.A., M.D.
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In a paper on the "Chrome-Silver Method," published in BRAIN, part lxxiii., p. 1, Spring Number, 1896, I described some small cells, which I found in the cerebellum, under the name of "granules with centripetal axis-cylinder processes." At the time I had found them only in two-day and four-day-old rats, but I have since found them in older rats, and am satisfied that they are not Golgi-cells in an embryonic condition, but structures which retain the same form throughout life. I find these cells near the summits of the folia and only in the deeper stratum of the granule layer. Their transverse diameter ranges from 10 μ to 15 μ (in the rat); their diameter is therefore about the same as that of an ordinary cerebellar granule.

Their axis-cylinders run parallel with the fibres of the arbor vitæ; often they traverse the granule layer for a very considerable distance before joining the fibres. In one case the axis-cylinder, after running towards the base of the folium for a considerable distance, loops upon itself, and returns towards the apex; owing to the irregularity of their course, it is often difficult to determine the destination of the axis-cylinders. Usually destitute of collaterals or side branches, they occasionally give off side branches which turn towards the molecular layer. Most of the cells are carrot-shaped, the horn of the carrot being often forked. Some of the cells are fusiform, a long process tapering from either end; the axis-cylinder then comes off from one of these two processes.

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The centripetal course of the axis-cylinder distinguishes these cells from the type of cell hitherto described as "granules" of the cerebellum in so marked a way that it may be desirable to describe them under a different name. Since Golgi was the first to describe the large nerve cells of the granule layer, it seems appropriate to name these cells "small Golgi-cells," although they are quite unlike any cells described by the Italian anatomist.

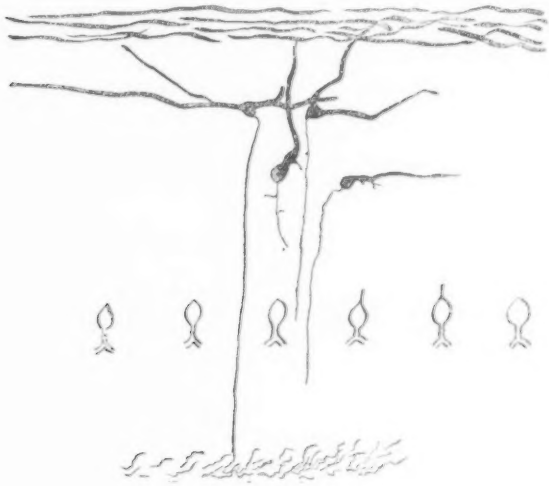

FIG. 1.

A group of carrot-shaped granules of the cerebellum with centrifugal axis-cylinder processes. The fibres of the arbor vitæ are seen at the top of the sketch. The cells of Purkinje are marked in outline. The axis-cylinders of the granules pass between the cells of Purkinje to bifurcate in the molecular layer. Section slightly oblique to the long axis of the folium. Kitten three weeks old.

Since finding the cells just described as small Golgi-cells, I have discovered, both in the cat and in the rat, cells almost identical with them in form (fig. 1), but provided with an axis-cylinder process, which bifurcates in the molecular layer. In the particular specimens examined, these

cells are slightly smaller than those (found in other subjects) with centripetal axis-cylinders, but otherwise they resemble them very closely. Their transverse diameter is 8 or 9 μ in a three-week-old kitten. They are carrot-shaped, the horn often forked, or double, or spindle-shaped, with the ends of the spindle of very considerable length. The dendrites (if we may apply this term to the tapering processes of these granules) usually incline towards the fibres of the arbor vitæ. The axis-cylinder is exceedingly delicate, comes off from the end or side of the cell, and makes its way between the cells of Purkinje to the molecular layer. Its course is almost straight, though undulating. Within the molecular layer the axis-cylinder process bifurcates after the manner described for the axis-cylinder processes of ordinary cerebellar granules by Ramón y Cajal and Pedro Ramón.

Dogiel[1] has recently described the cells of the granular layer as he finds them in the dove, with the aid of the methylene-blue method. He divides them into two classes —the "small cells of Golgi" and the "large cells of Golgi." The small cells he figures with more slender processes than the chrome-silver method shows them to possess. The cells near the cells of Purkinje are not unlike the granules as they ordinarily appear. The more deeply-placed cells are larger. As Dogiel gives no measurements and makes no statement on the subject, it is difficult to tell whether he regards these small cells of Golgi as identical with the "granules" which Golgi describes and figures—the well-known round granule with three or four arms, ending in bent claws or irregular patches—or as cells of a kind not hitherto described. In either case it appears to me to be a mistake to call a small cell with a centrifugal axis-cylinder a "Golgi-cell." These small cells are the "granules" of the cerebellum, of which the nervous nature was proved by Ramón y Cajal, when he discovered their axis-cylinders. The term "granule" is an extremely convenient term for cells of this type. It is a pity to confuse them with the huge cells with centripetal axis-cylinders, which were first described by Golgi, and are

[1] Dogiel, "Die Nervenelemente im Kleinhirne der Vögel und Saügethiere," *Archiv f. Mikr. Anat.*, xxxvii., 4, July, 1896.

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now very properly known by his name. Ramón y Cajal makes a similar protest (*Rev. Trim. Micr.*, I., p. 176).

Granules of the Olfactory Bulb.—It was the object of my investigation into the chrome-silver method to examine

FIG. 2.

Larger granule of the olfactory bulb. The axis-cylinder comes off from a central dendrite, courses peripherally almost to the mitral cells, and then folds back towards the bulbus ventriculi. It gives off six collaterals. Guinea pig, six weeks to two months old (half grown).

its credentials with a view to judging how far we can trust it as a witness on whose authority we are justified in accepting the conclusions, as to the plan of structure of the nervous system, of those who have chiefly worked with this method—

conclusions which are completely subversive of all that had been believed hitherto. Among these innovations was the introduction of the idea of the existence of "amacrine cells," *i.e.*, cells destitute of axis-cylinders. They are described particularly in the retina and olfactory bulb. The bulb seemed the easier situation in which to test their existence. The chrome-silver method ought not to be appealed to to prove a negative, but when it gives a positive result the picture is, fortunately, so clear as to admit of no dispute. After a long search I found the axis-cylinders of the granules, which I described and illustrated by a photograph in the paper referred to. These were granules of the form which I am about to describe as type 1, for I now find the granules of the olfactory bulb to be of two types :—

(1) The larger granules, of which fig. 2 is an illustration. The cell figured has a diameter of 14 μ. Its axis-cylinder turns at first towards the glomeruli, and then, looping upon itself, runs for a great distance in a central direction, giving off side branches. The total length of the traject displayed in this section is ·75 mm. The cell lies in the midst of the granule layer, and is best—as it seems to me—termed a "granule," for it contrasts in a marked way with the large nerve cells of the bulb, especially with the very big cells which I have termed "bracket cells." It is equally distinct from the cells of Golgi's type II. as figured by Golgi, Ramón y Cajal and Van Gehuchten. Although its dendrites are rather larger and more numerous than those of the granules of the second class, they resemble them in disposition, passing outwards as far as the mitral cells, and also inwards towards the ventriculus bulbi. Many other cells of the same kind are to be seen in the preparation; their axis-cylinders being invisible, no histologist would hesitate to term them "granules."

(2) The bulb also contains granules, which give off centripetal axis-cylinder processes of excessive tenuity (fig. 3). These axis-cylinders are very rarely coloured by the chrome-silver method, and I have not yet obtained a specimen with the axis-cylinder coloured and the dendrites well displayed at the same time. At present, therefore, I cannot say how

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many of the granules usually seen belong to the first class and how many to the second. The granules of the bulb present every gradation in size, from the minute granules (8 μ), in which I have seen the straight centripetal axis-cylinders, to the larger granules (14 μ), with tortuous axis-cylinders.

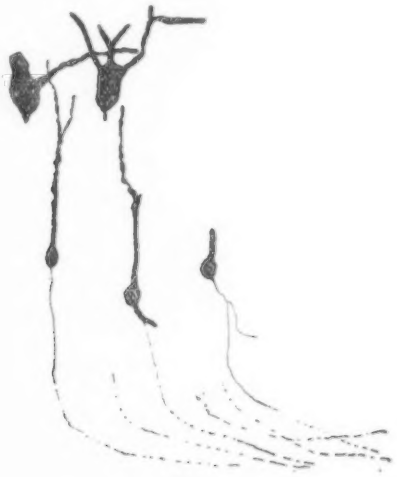

FIG. 3.

Three of the smaller granules of the olfactory bulb with centripetal axis-cylinder processes, and two mitral cells. Hedgehog seven days old.

The form of the granules of the bulb, the disposition of their dendrites, their thorns and their reaction to chrome-silver and other stains would have appeared to me to mark them as indubitably nervous, even if I had not discovered their axis-cylinders.

NOTE ON "THORNS," AND A THEORY OF THE CONSTITUTION OF GREY MATTER.

BY ALEX HILL, M.A., M.D.

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THE minute lateral branches born by the dendrites of all classes of nerve cells in the central nervous system were termed "thorns" by Ramón y Cajal. In their typical form they appear, in preparations made with the chrome-silver method, as minute stalks, each bearing at its end a black bead. Hence they have been called "gemmules," and the bead at the end (the "contact granule") has been supposed to be a dot of naked protoplasm, by means of which the dendrite establishes contact with nerve fibres which run parallel to itself.

Semi Meyer[1] has questioned the existence of thorns as real structures on the ground that he could not find them in preparations made by subcutaneous injection of methylene-blue. Kölliker[2] regards them as embryonic structures not to be found in the adult unless they appear as artifacts. Ramon y Cajal[3] has, however, shown that they can be displayed by methylene-blue staining both in the young animal and in the adult. He concludes that, since they are displayed by three such diverse methods as those of Golgi, of Cox, and of Ehrlich, there can be no doubt as to their existence.

[1] S. Meyer. Die subcutane Methylenblauinjection, ein Mittel zur Darstellung der Elemente des Centralnervensystems. *Archiv f. mikr. Anat.*, xlvi., 1895; und über eine Verbindungsweise der Neuronen *ibid*, xlvii., 1896.

[2] Kölliker. Handbuch der Gewebelehre des Menschen. Sixth edit., vol. ii., pp. 647, 755, &c., 1896.

[3] Ramón y Cajal. Las Espinas Colaterales de las Células del Cerebro teñidas por el Azul de Metileno. Revista Trimestral Micrográphica, vol. i., p. 123.

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I also find them in their typical form in nine preparations out of every ten, but I have nevertheless come to the conclusion that they are structures which are only partially revealed by either the chrome-silver or the methylene-blue, and that the typical form of a rod with a dot at the end is due to *post-mortem* change in the tissue.

I have examined the thorns in the brains of a large number of animals, young and old, all healthy, and all killed with chloroform, but hardened in various ways; and I find

FIG. 4.

Protoplasmic processes of pyramids of cortex cerebri showing thorns prolonged into filaments. *Adult hedgehog.* Blood vessels washed out with salt solution, dilated with 1 per. cent. lactic acid, and then injected with 4 per cent. formaline. Hardened in bichromate of potassium and formaline.

so great a variation in their appearance that I have come to the conclusion that they are organic structures which are not shown in their entirety by the chrome-silver method.

(1) They may be totally absent. I first noticed this in a brain injected through the aorta with warm bichromate of potassium and osmic acid solution, after washing out the blood-vessels with salt solution, and their dilation with 1 per cent. of lactic acid. They are present in certain other preparations injected in this way. I cannot, therefore, attribute their absence to the method. It is, however, very difficult to make sure that the irritating osmic mixture has penetrated all parts of the brain.

(2) When present they vary much in length, from less than 1 μ to 8 or 10 μ. The longest are found on the granules of the olfactory bulb.

(3) They present a considerable variety in form, the typical stalk and dot giving place to a filament with two or three dots on its course, or the filaments being invisible and

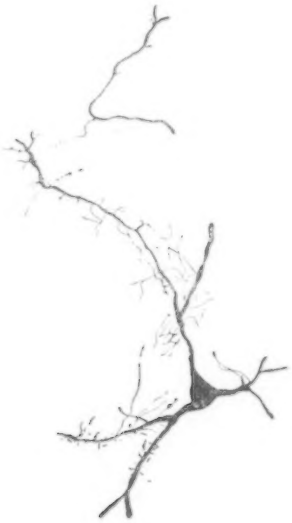

FIG. 5.

Cell of nucleus lenticularis. Dendrites bearing filaments in place of thorns. Adult hedgehog. Blood vessels washed out with normal salt-solution, dilated with 1 per cent. lactic acid, and then injected with 4 per cent. formaline. Hardened in bichromate of potassium and osmic acid.

the dots appearing alone, in a parallel row on each side of the dendrite.

(4) In certain specimens, as shown in figs. 4, 5 and 6, the thorns are replaced by filaments of varying length, which appear to be in direct continuity with the cell or its dendrites.

It is the main trunk and the large branches of the dendrites which usually carry lateral filaments in place of

thorns. The filaments are best seen (*a*) on the apical dendrites of large cortical pyramids, near to the cell, or (*b*) on the centripetal (or apical) dendrites of the double pyramids (giant pyramids) of the subiculum cornu ammonis. Two difficulties at once present themselves. (1) When the lateral filaments are not seen the trunk of the dendrite usually appears smooth and devoid of thorns. (2) In both the situations named (*a* and *b*) a vast quantity of tangential fibres cross the dendrites—in the cortex, the fibres of the laminæ

FIG. 6.

(1) A pyramid. (2) A cell of the nucleus lenticularis. Same animal and same method of preparation as 5.

medullares; in the cornu ammonis, the remarkable deep layer of "mossy" or rosette fibres. It is open to us, therefore, to reject the continuity of the dendrites and the lateral filaments on the plea that it is a delusive appearance, an instance of attractive staining, the colouration of heterologous and completely detached structures in the neighbourhood of coloured elements—a phenomenon with which we are not unacquainted when working with the chrome-silver method. On the other hand, the continuity is unmistakable, if we may trust

the method and the microscope, although we have to bear the reflection constantly in mind that it is impossible, when examining block structures, to distinguish between continuity and juxtaposition. It seems reasonable, however, to put the most obvious interpretation upon the preparations, and to accept their evidence that under certain conditions of staining the thorns may be replaced by filaments. As throwing light upon the connection of the thorn filaments with the trunk of the dendrite, it may be pointed out, that although this part of the dendrite is usually thornless, it has frequently a warty appearance. It may be that the filaments connected with the trunk of the dendrite are coarser than those connected with its twigs. Possibly they are derived from tangential fibres. The mossy fibres appear to be non-medullated. What becomes of the group of filaments given off at each rosette? No method of colouration has, as yet, shown their destination. They are usually figured as thick, short, and blunt, but often they are filamentous. Probably this is another illustration of the varicose accumulation of cell plasm, about to be given as an explanation of the dots at the ends of thorns.

These variations in form—taken in conjunction with the fact that in many mis-stained specimens the cell outline is invisible, owing to the mycelium of filaments by which it is surrounded, which makes it resemble a burr on a rose bush rather than the ordinarily well-defined nerve cell—lead me to believe that A THORN IS REALLY THE CELL-END OF AN UNSTAINABLE NERVE-FILAMENT SURROUNDED BY A FILM OF STAINING CELL-PLASM.

Nerve cells appear to me to consist, anatomically, of two substances, (1) the non-staining filaments and (2) the soft cell substance in which the filaments are embedded. This was the view of Max Schultze. It has been recently endorsed by Flemming, Rohde, and others. For the purposes of this paper, I will term the two constituents simply nerve fibrils and cell plasm, since at the present time I desire to express no opinion as to whether the fibrils are hollow or solid, or as to the constitution of the cell plasm. In the axis-cylinder the nerve fibrils are embedded in cell

plasm which is invested by some kind of sheath, hence the sharp outline of the axis-cylinder, by which it is always distinguished. The protoplasmic processes of the cell are naked.

Nerve fibrils brought to the association fields in grey matter are invested for a certain distance with the axis-cylinder sheath. Eventually the fibrils separate from one another, lose their sheaths, and, since they are not stained (by the chrome-silver or by the methylene-blue method), they become invisible in preparations made with the aid of these methods. They again come into view when they join the dendrites of nerve cells, owing to the overflow of the cell-plasm from the naked dendrite along the fibril. This portion of the fibril to which cell plasm adheres is the "thorn." Within the cell they are to be seen as filaments which traverse its substance on their road to its axis-cylinder process, by which they are collected into a bundle. It goes without saying that this transference of nerve fibrils occurs between heterologous, and not between homologous, neurons.

Cell bodies are often seen to give off filaments. It appears that the fibrils of afferent nerves enter the body of the cell as well as its processes. Probably the appearance of many forms of "basket" endings, described by Cajal, Held, Meyer and others, in which a cell body is grasped by thick, blunt, irregular fingers, is due to the varicose accumulation of cell plasm upon the brush of terminal fibrils into which the last branches of afferent nerves divide.

If my interpretation of the thorns is the right one, we are brought back to the theory of Gerlach, with certain modifications introduced by recent discoveries. It is tempting to speculate as to the mode of working of a central nervous system thus constituted, but in the present state of our knowledge, all hypotheses are mere speculations. It appears to me probable that the presence around the fibril of a film of cell plasm is necessary for the conduction of impulses. Possibly, the opening or closing of a nerve path depends upon the greater or less extension of this plasm film around the filament; an open reflex path being one in

which the film of cell plasm is permanently extended; inhibition being due to the withdrawal of the plasm into the cell.

The need which the psychologist constantly observes of a condition of "attention" on the part of an association field, if an afferent impulse is to produce its full effect, would appear to be due to the fact that the impulse (or rather sequence of impulses, for I imagine that all impulses are vibratory) has, on reaching the cell, to induce the overflow along the fibril of the cell plasm which favours its passage. One who adopts this theory of continuity, might easily amuse himself in formulating hypotheses which would constitute a complete system of nerve physiology; but it is hardly worth while until the anatomical basis of the theory has been established.

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THE MUSCLE-SPINDLE[1] UNDER PATHOLOGICAL CONDITIONS.

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ALTHOUGH the existence of the spindle in muscle has long been known, and a considerable amount of literature has been accumulated by continental writers, yet until a publication by Sherrington (30) in the *Journal of Physiology*, the subject had received but small attention in England. Beale (2), indeed, in 1862, gave an accurate description of the spindle, but regarded it as muscle and nerve in the course of development. It is my purpose, therefore, in the following paper:—

(I.) To give a short account of the work already done on the muscle-spindle.

(II.) To enumerate the various views that have been held with regard to the origin and function of the muscle-spindle.

(III.) To give a short account of the methods I have found to be useful.

(IV.) To give a description of the muscle-spindle as found in man.

(V.) To demonstrate the condition of the muscle-spindle under the following pathological conditions:—

(1) Infantile paralysis.
(2) Tabes.
(3) Myopathy.
(4) Progressive muscular atrophy.
(5) Peripheral neuritis.
(6) Injury to the Brachial plexus.
(7) After section of the sciatic nerve in cats.

[1] Synonyms Muskelknöspen, umschnürte Bündel, Faisceaux neuro-musculaires, Neuritis fascians, Neuro-musculaere Stämmchen.

(I.) History.

In 1843, Miescher (21) described, in the abdominal muscles of a house mouse, bodies to which he gave the name of *Schläuchen*. V. Siebold (31) figures these in a paper published in 1853, and states that the same bodies have been found in rats. Miescher thought they were pathological or a parasite, a view which v. Siebold, however, negatives. It seems to me not improbable that these were bodies which one now knows as muscle-spindles.

In 1861, Weismann (37), working with the muscle of frogs, described very fine muscle fibres in groups of six, eight, ten, or more. The diameter of these fibres varies considerably. They are bound together by a cord about the centre of their course, and, indeed, appear to be enveloped in a dull granular substance, through which one can but indistinctly trace each fibre; these fibres are equal in length and reach from tendon to tendon. Weismann considers these fibres as a stage in the development of muscle.

In 1862, Kölliker (18) described his *Muskelknöspen* which he found in the muscle of frogs, and he believed them to be muscle in the process of division. In his Croonian Lecture, 1862 (18), he says that though at first sight these bodies appear to be of the nature of tactile corpuscles or terminal nerve bulbs, they do not really belong to that class of body. They are nodular swellings marked by a profusion of rather roundish nuclei, and receiving a single very thick nerve fibre loosely surrounded by its comparatively wide membranous sheath. On further examination it is found that the nerve fibre, on reaching the nodules of the muscle, is wound up into a coil, in the meantime undergoing repeated division, and it may be seen to enter the muscle fibre.

On careful examination, however, it is found that the apparently simple muscular fibre is really a small bundle of three to seven fibres, and that the penetrating nerve fibres pass between these muscle fibres. Simultaneously with the increase of its terminal fibres, the dark-bordered parent nerve fibre doubtless augments in thickness, which explains the fact, otherwise difficult to understand, that it is invariably of

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much larger size than the nerve fibres proceeding to the other muscular fibres.

In 1889 (18), Kölliker had studied the muscle-spindle not only in frogs, but also in rabbits and man, and after discussing the various views held with regard to these bodies, he still holds that they are muscle in the course of development.

In 1862, Beale (2), in a paper on the distribution of nerves to the elementary fibres of striped muscle, gives a detailed description of these bodies, and regards them as muscle and nerve in the course of development, and it is from observation of these that this author comes to the conclusion that the muscle fibre is supplied with nerves in its entire length.

In his Croonian Lectures, 1865 (2), he further considers these bodies, holding the same view and differing only from Kölliker in that he believes the development of the muscle-fibres takes place from the nuclei and not by division of the parent fibre.

Kühne (19), in 1863, working with the muscle of white rats, described a nerve dividing into two branches and entering a spindle-shaped muscle, where it ended in clear, transparent bodies, the nerve actually ending in a distinct hillock. In a more detailed paper on "Die Muskelspindeln" (19), he says that the above-described endings correspond with the broad nerve fibres. The proportion of spindles to muscle fibres is about 1-100 (in house mouse), and he further remarks that the abdominal and thoracic muscles seem to have more than the extremities. The nerve fibres passing to the spindles are three or four times as broad as those to motor nerve-endings. Each spindle is, as a rule, supplied by two nerves which come from a common trunk, the sheath of the nerve fibre passing into the sheath of the spindle. The muscle fibres of the spindle are smaller than the neighbouring fibres, and have well-marked striations at the ends, but lose these towards the centre; the striation of the muscle is broader than in the larger fibres, but this is usually the case with the smaller fibres. In conclusion he says: "It would seem from the above that a knowledge of the muscle-spindle is of great importance in

understanding the development, regeneration, and the growth of muscles and their nerves.

In a further paper (19), published in 1864, he says: "Are they an apparatus with a peculiar, still unknown physiological function for the *Zuckungsvorgang des Gesammtmuskels*, or are they only a stage in the not yet completely developed muscle fibre?"

Peremeschko (23), in 1863, in studying the development of striated muscle, discusses the spindle described by Kühne, denies that a nerve enters them, and agrees with Kölliker in calling them *Muskelknöspen*.

Sachs (28), in 1874, in a series of papers on the sensory nerves of muscles, shows that in frogs after section of the anterior roots for a period of six to eight weeks the motor fibres were completely degenerated, on the other hand the nerve in the muscle contained two well-formed, broad, medullated fibres, which after dividing gave off coarse nerve twigs; the further distribution of these differed entirely from the condition of a motor nerve.

He then discusses the result of section of the posterior roots peripherally to the ganglion. The degeneration of the sensory muscle nerves takes places very slowly, and after six weeks the appearance is by no means striking, and on this point he comes to no definite conclusion. He describes the nerve entering a spindle-shaped body and twisting round the muscle fibre in a spiral form.

Eisenlohr (9), in 1876, described, in a case of infantile paralysis, a small number of well-preserved fibres arranged within a sheath which they incompletely fill. He does not seem to have recognised them as spindles in cross section and would seem to regard them as pathological.

Fraenkel, in 1878 (13), in an exhaustive paper on the changes in the muscle of phthisical patients, describes "umschnürte Bündel" situated generally in the neighbourhood of vessels and nerves. He considers them pathological. He gives measurements of these bundles and there is no doubt that he is describing the muscle-spindle in transverse section. He states that he has never found them in the muscles of the eyes, never in the diaphragm or masseter, but they are

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abundant in the muscles of the thumb. He found these "umschnürte Bündel" in one-third of all his preparations.

Ranvier (25), in 1878, discussing the spindles described by Kühne, says that they are cylindrical in shape. He notes that the muscle fibre loses its striation and becomes granular, shiny, and contains several nuclei at one part. He calls attention to the resemblance of the sheath to that of a Pacinian body. He says the nerve, after penetrating the sheath, divides into two; some fibres pass between the layers of the sheath, others either pass along a muscle fibre or curl spirally round it. He does not know whether they should be considered muscle fibres in the course of development or fibres undergoing atrophy.

Millbacher (22), in 1882, examined muscles in thirty cases of acute and chronic disease; he, like Fraenkel, found the "umschnürte Bündel" in about one-third of all his preparations; he, too, never found them in the eye muscles, diaphragm or masseter, being abundant, however, in the thumb. He describes three forms of "umschnürte Bündel":

(i.) Unvollständig umschnürte Bündel;
(ii.) Vollständig umschnürte Bündel, containing muscle fibres;
(iii.) Vollständig umschnürte Bündel, containing atrophied fibres;

and in these three forms he sees the process of degeneration. He found them also in four cases of apparently healthy individuals, and in these cases he is doubtful of their significance.

Golgi (15), in 1882, describes a spindle and gives drawings in cross and longitudinal sections. Why these fibres lie imprisoned and preserve, even in adult life, the characters of imperfectly developed fibres is not clear. He suggests that they have some special connection with the lymphatic system.

Bremer (4), in 1883, working with the muscles of frogs, mice and lizards, believes that muscle-spindles are a stage in the development of muscle, and he traces the various steps in development, not only of the muscle, but also of the end

plate. He notes the large size of the nerve fibre entering the spindle and its thick sheath; he notes that the muscle fibre loses its striation near the centre of the spindle. He gives a detailed description of the spindle, and associates the variation in the spindle with various stages of development.

Babinski (1), in 1884, described, in the muscles from a case of chronic myelitis, circular rings having in their centre several atrophied fibres. He considered them as pathological. In a further paper, published in 1886, he gives a description of the spindle in transverse section, having a diameter of 100 to 200 μ; he says the spindle generally contains three to seven fibres; he notes the sub-division of the intrafusal space, one part conveying the vessels, another the nerve, and the third the muscle fibres. He no longer considers them as pathological, but is doubtful of their real meaning. In a third paper, published in 1889, he criticises Eichhorst's "Neuritis fascians."

Mays, in 1884, studying the distribution of nerves in the muscles of frogs by means of the gold chloride method, describes certain appearances which he compares to the muscle-spindles of Kühne; he believes they are in connection with the sensory nerve fibres.

In 1892, in a paper on the development of the motor nerve endings, he expresses the opinion that muscle-spindles take no part in the development of muscle.

Roth (26), in 1887, published, under the heading "Neuromusculaire stämmchen," a description of a muscle-spindle. He had found them first in atrophied muscle, and soon after in healthy muscle. He considers them as physiological organs.

Westphal (39), in 1887, in two cases of pseudo-hypertrophy in sisters, describes small groups of muscles surrounded by connective tissue occurring in muscle which was completely atrophied; he does not seem to recognise them as muscle-spindles.

Eichhorst (8), in 1888, describes, under the heading "Neuritis fascians," certain structures he has noted in the muscles from a case of alcoholic neuritis. He describes the onion-like sheath of the spindle, and believes that it comes from the thickened sheath of the nerve.

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Felix (11), in 1888, working with the muscle of fœtuses from the fourth month to the thirty-sixth week, describes "primitive muscle bundles"; these are characterised by (1) greater breadth, (2) more intense staining, (3) wider striation, (4) marked Henle sheath. Nuclear increase is marked at one part of the muscle fibre, and tracing a primitive bundle one sees (1) an increase of irregularly-arranged nuclei, (2) nuclei arranged in rows; usually three rows of nuclei are found, the primitive bundle here beconing widened to nearly three times its former diameter. Only once did he find a nerve near, and he doubts whether they had any connection; he compares them to the muscle-spindles of the frog. In a further paper (11), in 1889, studying the growth of striated muscle in man, he points out that in the adult muscle-spindle there are usually three to six muscle fibres, whereas in the fœtal spindle there may be as many as twenty; this he attributes to the division of the daughter fibres. The sheath of the spindle is characteristic, and passes gradually into the perimysium of the muscle, so that on section towards the end of a spindle there is nothing to call attention to the presence of a spindle. He believes that spindles are most frequently found near the tendon, and, as may be inferred from the above, he considers them as a stage in the development of muscle.

Cattaneo (5), in 1888, studying the musculo-tendon nerve termination described by Golgi, both under normal conditions and after experimental lesions in guinea-pigs, rabbits, cats, and dogs, describes a body having a length of 80 to 800μ. situated generally where the muscle passes into the tendon, one extremity of the organ passes into the tendon the other into the muscle. The sheath enclosing these resembles the Henle sheath of a nerve. A nerve enters this body, and after passing through the sheath divides into two; the mode of termination is described. Blood vessels can also be seen entering this body. Among these musculo-tendon organs one can sometimes see a muscle-spindle, such as has been described by Kölliker, and after discussing various views, he is inclined to agree with him (Kölliker) rather than with Millbacher and Kraske, who regard them as pathological.

He has seen the muscle-spindle lie between two of the

musculo-tendon organs, but the nerve supplying the spindle arrives from a different direction. There is, however, a certain analogy between the two. With regard to the function of these musculo-tendon organs, after discussing the point at some length, he says that if they are the organs of muscular sense they ought to be independent of motor nerve fibres, and to be intimately connected to sensory fibres. To answer this question two paths are open: (1) the pathological, (2) the experimental. Not having the opportunity for the first, he turns to the second.

In dogs, after section of the posterior roots (fifteen days to four months), ataxia was found to be present, but he could find no alteration in the musculo-tendon organs nor in the muscle fibre.

In dogs in whom the anterior roots were cut the musculo-tendon organs remained normal; the muscle itself, however, showed advanced atrophy.

After complete section of the nerve, changes were found in the terminal organs 36 to 38 hours after section.

In conclusion, he says that the musculo-tendon organs are found sometimes in contact with muscle-spindles, sometimes with Pacinian bodies, but the position of the nerve fibre proves them to be independent of these bodies.

He considers the musculo-tendon organs above described as the true organs of muscular sense.

He further adds that, after section of the nerve, degeneration of the medullated portion of the nerve is slow, whereas in the non-medullated part it occurs in 20 hours, and disappears in three to four days.

Siemerling (32), in 1888, in a paper on a case of gumma of the base of the brain, describes what he considers to be a spindle in the inferior rectus of the eye, which muscle had undergone considerable atrophy.

In a second paper he criticises Eichhorst's "Neuritis fascians," and in a third (32) he reports a case of alcoholic neuritis, and says that the spindle seems to enjoy a certain immunity against pathological changes. This is not always so, however, for in two cases of phosphorus poisoning he found the muscle fibres of the spindle filled with fat globules.

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Kerschner (17), in 1888, after describing the muscle-spindle, says that the spindle frequently ends in the connective tissue, and suggests that the function of the spindle might be to protect the nerve-ending; he comes, however, to the conclusion that they are a complicated sensory organ which may serve muscular sense. In a further paper (17) he compares muscle-spindles with other sensory end organs found in joints, tendons, conjunctiva, tongue, &c. In 1892, von Ebner (17) showed for Kerschner certain specimens to demonstrate the nerve-ending within the spindle stained with gold chloride; two nerve-endings are described, one a motor on the muscle fibre, another sensory, twined round the muscle fibre. In support of the sensory nature of these organs he puts forward the following points:—

(i.) The abundant nerve supply, far above the number of muscle fibres.

(ii.) The division of the nerve fibre after a type which reminds one more of a sensory than of a motor ending.

(iii.) The presence of nuclear groups like those of end-bulbs.

(iv.) The frequent ending of part of the same nerve in a tendon organ.

(v.) The analogy with a Golgi organ: (*a*) supply from the same nerve; (*β*) similar sheath.

(vi.) Their constant presence; their number and appearance in many muscles in which fine muscular sense is needed.

(vii.) The apparent vicariousness of the Golgi organ in similar muscles, as in the muscles of the eye.[1]

Kerschner then enters into the various points that have been advanced in proof of the embryological characters of the spindle, viz., the darker staining, the abundant nuclei, the variations in the sheath, and the presence of double and quadruple spindles. He says the darker staining frequently occurs with the smaller muscular fibres, the abundant nuclei occur at the nerve entrance, and the variations of the sheath are dependent on the point at which it is cut across. He points out that mitosis is absent. The one point which he

[1] Comp. Marchi, *Archiv für Ophthalmologie*, xxviii., 1882, and *Journal of Physiology*, vol. xvii., p. 248.

agrees looks like a developmental process is the division of the spindle into two and sometimes into four. In a further paper (17), 1893, he sums up the views held at that date by various authors.

Pilliet (24), in 1890, described the presence of muscle-spindles in the muscles from cases of alcoholic paraplegia, chronic rheumatism, and amyotrophic lateral sclerosis. He says it would seem that atrophy of the muscle makes them apparent.

Bloeq and Marinesco (3), in 1890, showed that in a case of poliomyelitis and another of polyneuritis that the muscle spindles existed when there was total loss of all other muscle fibres.

Von Franque (14), in 1890, maintains the view that the muscle-spindle is a stage in the development of muscle.

Santesson (29), in 1890, describes the muscle-spindle in a case of myopathy (Leyden's form), and regards them as pathological appearances.

Dogiel (7), in 1890, studying the motor nerve-endings in the muscles of frogs, &c., by staining them with methylene blue, describes the entrance of the nerve into a muscle-spindle. The chief fibre to the spindle he describes as dividing into two—(i.) winds in the spindle in a spiral form and (ii.) distributes itself to the spindle sheath.

Christomanos and Strössner (6), in 1891, studying the spindle under normal circumstances in a fœtus, a new-born child, a child nine years old, and in an adult, found them in all of them, and they give a table showing the comparative size of the intra- and extrafusal muscle fibres. They describe an inner and outer sheath, and note that the spindle is larger in adults than in children, the increase being due chiefly to the thickness of the wall. They consider the spindle to be a sensory organ.

Trinchese (35), in 1891, sums up what is known of the muscle-spindle; he refers to his own work in 1888, when he demonstrated that the spindles had a different form and structure at the various periods of their development. He describes the nerve entering the nucleated part of the muscle fibre, and finishes by stating that the assertion of Bremer

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that these fibres are muscle fibres in the course of development has not yet been demonstrated as being true. It would, indeed, seem probable, but until the phase of transition between the adult muscle fibre and the spindle has been demonstrated it cannot be asserted positively what the nature of these organs is.

Erb (10), in 1891, describes a bundle of small fibres surrounded by a nucleated, definitely striated band, which does not completely surround these fibres (p. 94), and in relation to the regeneration taking place in a degenerated muscle, he says that the foregoing curious condition stands in close relation to the *Muskelknöspen* (p. 147).

V. Thanhoffer (34), in 1892, asserts that the so-called muscle-spindles are nothing more than "building material" for the regeneration of muscle and nerve fibres.

Volkmann (36), in 1893, studying the regeneration of striated muscle, says he has found the muscle-spindle in four cases—(i.) the thumb muscle after injury to the arm, (ii.) diphtheritic paralysis, (iii.) in rectus abdominis of man aged 40, and (iv.) in the hand of a man who died of spinal disease. He believes that these bodies have to do with the growth of muscle, and not with regeneration of muscle.

Ruffini (27), in 1893, compares the muscle-spindles as found in cats with those found in man. He gives a description of the various terminations, and states that the annulo-spiral termination present in the cat is not found in man. The termination *à fleurs* is present in both, but is more numerous in man. In conclusion, he says that one may regard these *fuseaux musculaires* as special nerve organs, having a function unknown.

Forster (12), in 1894, working with the muscle from cases of muscular atrophy, transverse myelitis, &c., found that in muscles which were completely degenerated the muscle fibre inside the spindle remained well preserved—the nerve fibre passing to the spindle being also well preserved, as shown by Weigert's staining. It would seem that the trophic centre for the spindle does not lie in the spinal cord, and it is suggested that it lies in the spinal ganglion. Muscle-spindles are considered as a physiological apparatus,

and the suggestion of Kerschner is followed, viz., that they serve muscular sense.

Sherrington (30), in 1895, examined muscle in which all the motor fibres had been divided and the muscle fully degenerated; he found that the spindle remained intact, the nerve fibre passing to it was well-preserved and could be traced to the sensory roots. The intrafusal fibres were well preserved, the striation well marked.

Further, in a cat in whom the sciatic had been divided for 150 days, the muscle was completely degenerated; with regard to the spindle, the nerve fibres passing to it were degenerated, and the spindle itself contained globules of fat, but the intrafusal muscles were well preserved and their striation well marked. It would seem, therefore, that the intra-fusal muscle fibres are independent, in regard to their nutrition, both of afferent and efferent fibres.

Sihler (33), in 1895, published a method of staining spindles with hæmatoxylin. In the same year he published the results of his investigations in snakes and frogs. He gives a detailed account of the spindles, and considers them to be sensory organs.

Weiss and Dutil (38), in 1896, working with the muscle of rabbits, cats, &c., and using by preference the gold chloride method described by Ranvier, describe three modes of nerve termination within the spindle:

(i.) Two fine branches, one passing above, and the other below, a nucleus.

(ii.) A small eminence on the sarcolemma, where there is a group of two to three nuclei. This ending is generally found toward the pole of the spindle.

(iii.) Fibres spread themselves on the surface of the muscular bundles in tree-like form, deprived of all nuclear elements.

They arrive at the conclusion that spindles are sensory in function, and probably have the same function as Golgi's tendon organs.

Gudden (16), in 1896, says that in alcoholic neuritis the muscle fibres in the *neuro-musculaere Stämmchen* are degenerate, and that the nerves within the spindle do not stain normally.

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(II.) Theories with regard to Origin and Function of the Muscle-spindle.

From the above abstract it will be seen that various views have been held as to the origin and function of the muscle-spindle, and I would here simply enumerate them.

(i.) That the muscle-spindle is muscle and nerve in a stage of development.

(ii.) That the muscle-spindle is muscle in the state of degeneration; (*α*) physiological; (*β*) pathological.

(iii.) That the muscle-spindle is a sensory nerve-ending in muscle.

(iv.) That the muscle-spindle is a protection to the nerve-ending during contraction of the muscle.

(v.) That the muscle-spindle has special connection with the lymphatic system.

I do not propose in this paper to enter into a discussion of these theories, all of which are dealt with at some length in the papers above quoted.

I believe, however, that all the evidence at present points to the fact that the muscle-spindle is the organ from which muscular afferent impulses are derived. Closely allied to the muscle-spindle are the musculo-tendon organs, and the tendon organs situated respectively between the muscle and the tendon and in the tendon.

What the exact nature of the impulses derived from these organs may be seems to me at present uncertain, but I think that the suggestion of Kerschner with regard to the muscle-spindle, by Cattaneo with regard to the musculo-tendon organ, that they are connected with the sense of position, may well receive consideration.

(III.) Methods.

I have used the following methods and stains in my work:

As often as possible portions of muscles have been hardened in Müller's fluid (some specimens which had been in spirit for some time were not put into Müller's fluid).

Small pieces were then placed in Marchi's solution for five to six days, washed in water, placed in alcohol and embedded in celloidin—some were, however, embedded in wax, cut and mounted in series.

Staining.

(i.) Specimens were mounted without further staining.
(ii.) Others were stained with hæmatoxylin and eosin.
(iii.) Others were stained by Pal's method.

The method, however, which I have found of most service is that described by Sihler (33).

A portion of fresh muscle is taken, and I find it best to select a part near the entrance, or slightly below the entrance, of a nerve. Divide this longitudinally into pieces the size of a small pencil, or, as I have found very convenient, freeze the tissue and cut *thick* sections in the direction of the muscle-fibre—put these into the following solution :

Acetic acid	...	...	1 part
Glycerine	...	...	1 part
Chloral hydrate solution, 1 per cent. in *distilled water*	...	...	6 parts

Let the tissue remain in this solution for twenty-four hours, then saturate with glycerine for two to three hours. The tissue is then swollen up, and can easily be pulled apart into fine strands about the size of a pin (if the tissue has been cut on a freezing microtome this is not necessary), which are placed in the following solution for three to ten days :

Ehrlich's hæmatoxylin	...	...	1 part
Glycerine	...	...	1 part
Chloral hydrate sol. 1 per cent. in *distilled water*	...	...	6 parts

Pieces of muscle are now removed, placed between two slides and squeezed ; on holding the specimen up to the light, or on looking at it under a low power of the microscope, a spindle-shaped body can often be seen stained darker than the surrounding muscle fibre. It will be noticed, too, that

the nerves, vessels, and lymphatics are also stained darker than muscle tissue. The spindle can now be teased out under the low power of the microscope, or even without the aid of any magnifying power.

In searching for the spindle it is always a good plan to follow a nerve fibre, as it frequently happens that such a fibre guides one to a spindle.

Having separated out a spindle, it can now either be mounted in the usual way or, if overstained, treated with dilute acetic acid—or after washing in water can be put to harden in Müller's fluid.

After hardening in Müller's fluid till all colour has disappeared from them, they can be placed in Marchi solution, treated in the usual way, and embedded in celloidin, and cut either longitudinally or transversely *in series*. The sections can then be stained by Pal's method, and a counter stain, dehydrated, and mounted in series. Specimens hardened in the above method, and mounted without further staining, show any recent degeneration.

Many of the series have been cut in wax, but I have experienced great difficulty in staining a spindle by Pal's method as a whole, and by far the most satisfactory results are obtained by cutting in celloidin and staining each section separately. The embedding in wax is, however, very useful for pieces of muscle stained with carmine or with Marchi solution, which do not require further staining.

(IV.) The Normal Muscle-spindle as Found in Man.

Distribution.—Muscle-spindles have been found in nearly all muscles of the body, but they are by no means evenly distributed. They are certainly numerous in the small muscles of the hand, and in the biceps of the arm (the latter muscle I have used more often than any other one muscle). Forster (12) states that they are frequent in the extrinsic muscle of the larynx. I have been unable to find them in the muscles of the eye, the intrinsic muscles of the tongue, or in the diaphragm, but my examination of these muscles

has been by no means exhaustive. The above statement agrees with that of other authors, Fraenkel (13), Millbacher (22), Kerschner (17), and Sherrington (30); but, with regard to the eye, is at variance with the statement of Siemerling (32), who has described a spindle in an atrophied inferior rectus muscle of the eye; and in regard to the tongue is at variance with a statement of Forster's (12), that they are to be found in the hinder part of the tongue.

Further, the muscle-spindles are more frequent in the belly of the muscle than near the tendon, but in relation to this statement it would seem to me that we should recognise in the muscle-spindle, in the musculo-tendon organ, and in the tendon organ the gradual transition from the one organ into the other, and I would regard them, not as distinct organs, but as variations of the same organs adapting themselves to the tissue in which they are situated, and in all probability serving the same function.

Age.—Muscle-spindles occur at all ages, from the fourth month of fœtal life onwards; this has been shown by Felix (11), Siemerling (32), and Christomanos and Strössner (6).

Size.—The size of a muscle-spindle varies considerably. One of the longest simple spindles I have found measured 11·7 mm., and the maximum breadth ·5 mm., but an average size is from 2 to 4 mm. in length, and ·15 to ·4 mm. in breadth. It would seem to me that the size varies both with regard to age and to the length of the muscle, being longer in the adult than in the child, and in the longer muscles.

Frequency.—I have made no attempt to estimate the number of spindles in any given muscle, but an estimate has been made by Felix (11), who found seventy-nine in one biceps. They are, however, much more easily found in wasted muscle than in well-developed muscle, and more easily in the muscle of a child than in that of an adult, hence the muscle of a child dying of a wasting disease is a muscle in which it is easy to demonstrate the muscle-spindle.

Description.—The muscle-spindle is, as its name implies, spindle in shape (fig. 1), varying considerably in size.

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PLATE I.

Fig. 1.

Normal Spindle from Abductor Pollicis of Foot (Man).

Length	..	..	..	..	..	..	..	..	6·65 mm.
Breadth	..	..	..	..	..	..	..	..	·38 mm.

A nerve (N) can be seen entering the equatorial region and this nerve contains a fibre whose breadth is ·01 mm. A branch containing finer nerve fibres, ·004 mm., leaves the nerve and enters the spindle further to the right. Another nerve (N) enters the spindle at the opposite pole, this also is composed of fine fibres—one measuring ·006 mm.

An artery (A) and a vein (V) can also be seen giving branches to the spindle.

Teased Specimen. Sihler's Method.

Magnified 8 diameters.

Fig. 2.

Compound Spindle.

Length of complete spindle	..	..	..	..					1·5 mm.
Length between entrance of nerves	..	..	..						1·275 mm.
Breadth	..	..	..	..	..	..	..	..	·12 mm.

The pole of the second spindle enters the other spindle a little to the polar side of the equatorial region.

Compare figure 15, compound spindle in cross section.

Teased Specimens. Sihler's Method.

Magnified 24 diameters.

Fig. 3.

Double Spindle.

Length ..	..	..	..	..	..	..	..	..	5·5 mm.

The pole of one spindle passes into the pole of the next, thus forming one continuous spindle—the nerve, however, enters the equatorial region of each spindle.

Teased Specimen. Sihler's Method.

Magnified 7 diameters.

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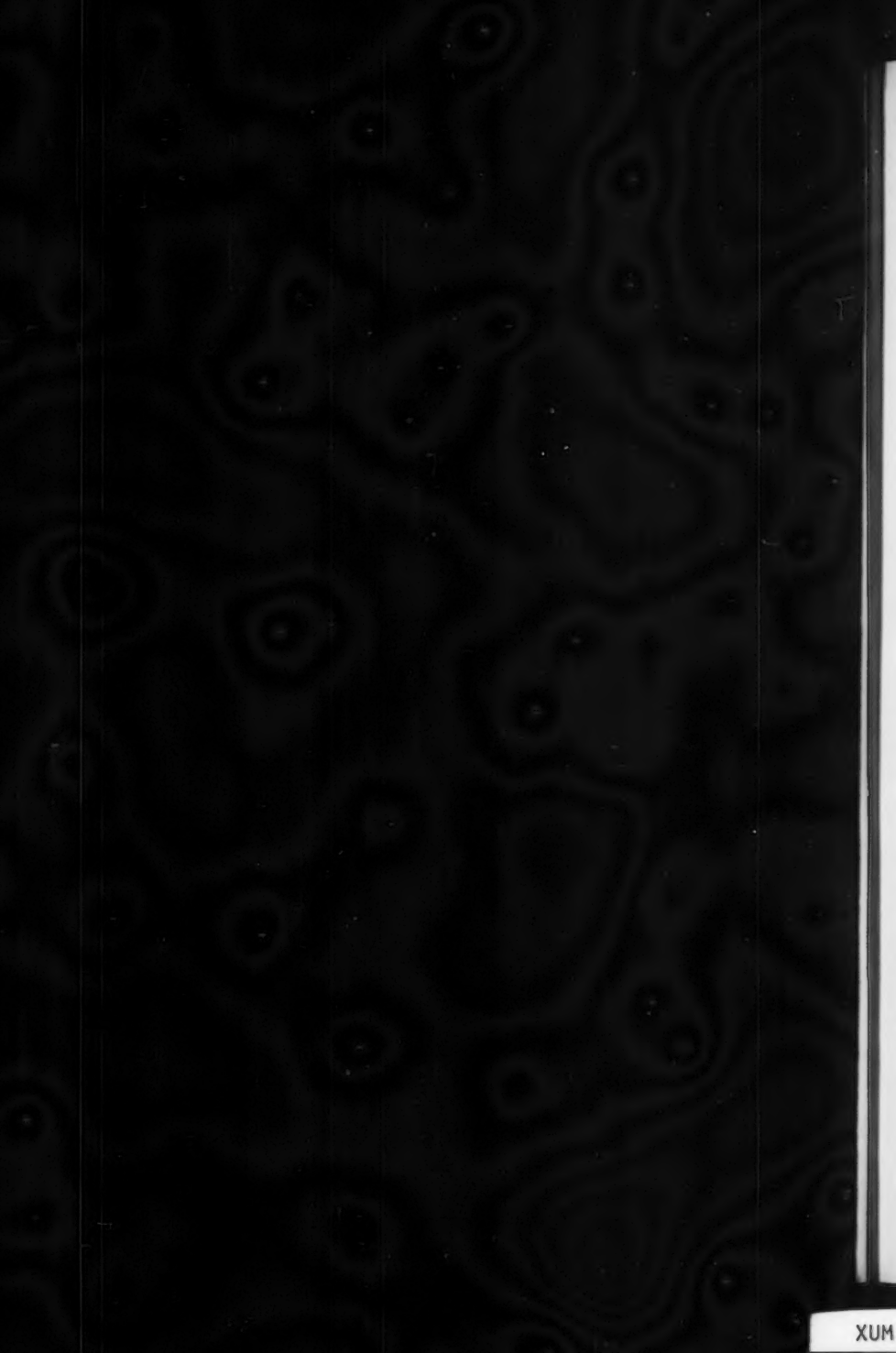
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PLATE I.

FIG. 1.

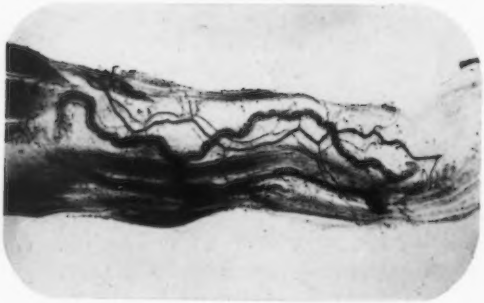

FIG. 2.

FIG. 3.

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It is common to find several spindles in one plane of the muscles, and they are often found along the course of a nerve from which they receive their supply, and lie parallel to it (fig. 2). Spindles are sometimes found compounded of two simple spindles (fig. 2), but I have no teased specimen of more than two so compounded. Sherrington, however, describes a triple spindle, and Kerschner a quadruple (fig. 15). On the other hand, it is not uncommon to find one spindle joined at its end with another spindle (fig. 3), and that again may be joined to another; three in a row is the maximum number that I have found. In such a case each spindle receives its own nerve supply.

The relation which the muscle-spindle bears in position to the muscle fibre also varies. Firstly, the muscle-spindle may lie wholly in muscular tissue, the muscle fibre passing in at one end, and, after passing through the spindle, passing out at the other. Secondly, the spindle may lie partly in muscular tissue and partly in connective tissue. Thirdly, the muscle-spindle may lie wholly in the connective tissue, the contained muscle fibre being independent of external muscle fibres.

The essential portions of a muscle-spindle are—(i.) the muscle fibres, (ii.) the nerves and nerve-endings, (iii.) the sheath, (iv.) the blood vessels, (v.) the lymphatics and lymph spaces, (vi.) septa dividing the spindles into compartments.

The various parts will now be described in the above order.

The Muscle Fibre.—One or more muscle fibres enter one pole of a spindle; these fibres are smaller than the ordinary muscle fibres, being about ·02 mm. in diameter (the average extrafusal fibre measuring about ·06 mm.) (fig. 4). The muscle fibres are well striated, and their striation is generally slightly coarser than the extrafusal fibres, ten striations occupying the same space as thirteen striations of the extrafusal fibres. As the muscle fibre or fibres pass towards the equatorial region of the spindle they become divided into many smaller fibres, some of these measuring only ·008 mm. (fig. 4). At a certain point the muscle fibre appears to lose its striation, and nuclei appear in the

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PLATE II.

FIG. 4.

Normal Spindle from Biceps of Man.

	Diameter.
Transverse section	·24 mm.
Extrafusal-muscle fibres	·04—·06 mm.
Intrafusal-muscle fibres	·008—·024 mm.

Magnified 160 diameters.

FIG. 5.

Normal Spindle in Longitudinal Section.

To show intramuscular nuclei, the nerve passing to this portion of the spindle is seen at N.

The sheath is well seen, and the comparative size of the intrafusal (Mi) and the extrafusal (Ne) muscle fibres is readily observable.

Intramuscular nuclei	·006 mm.
Intrafusal muscle fibres	·02 mm.
Extrafusal muscle fibres	·04 mm.
Nerve fibre	·004 mm.

Magnified 110 diameters.

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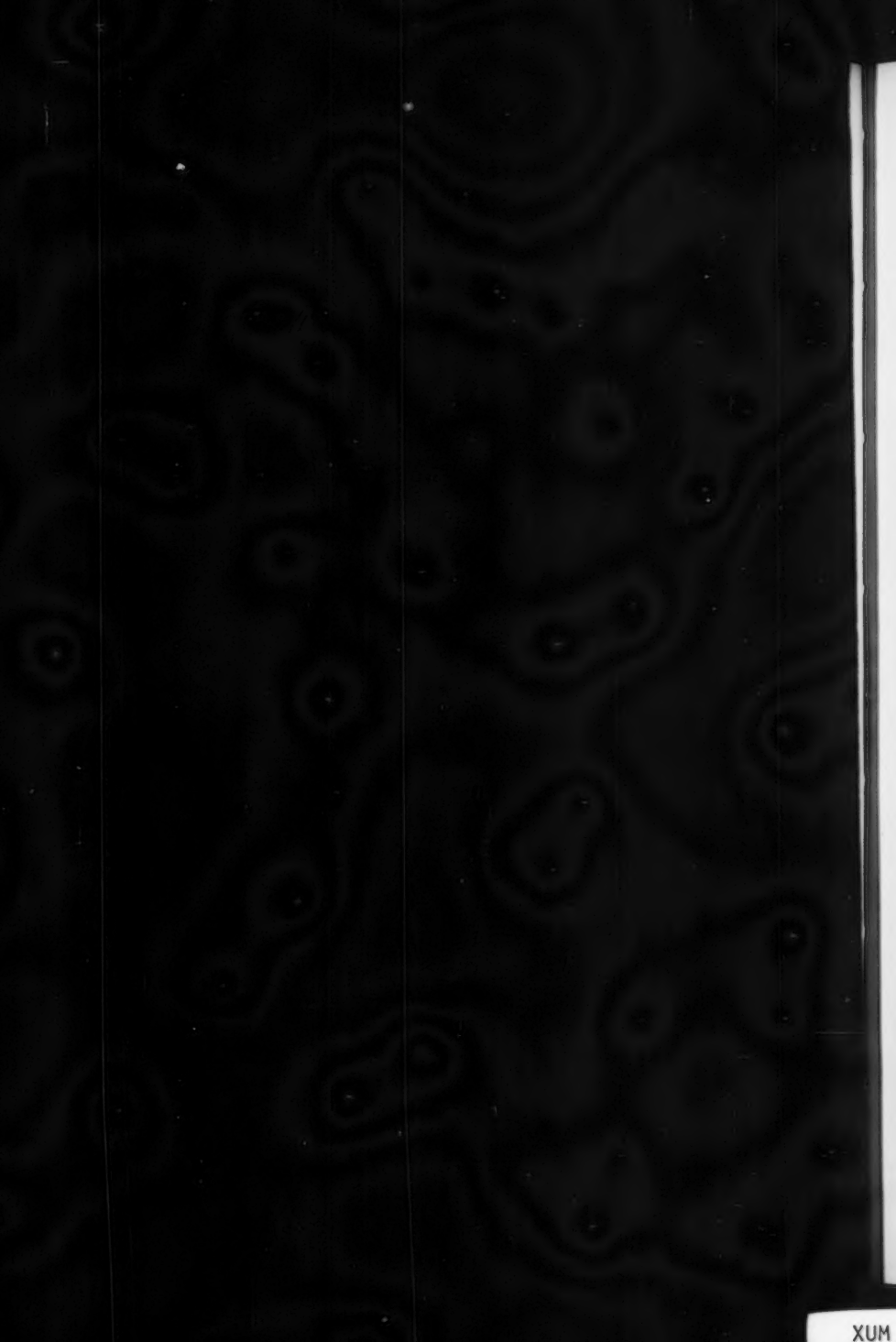

PLATE II.

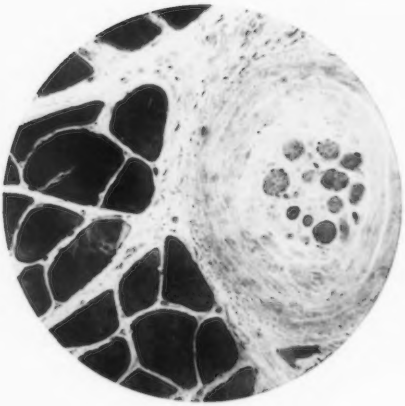

FIG. 4.

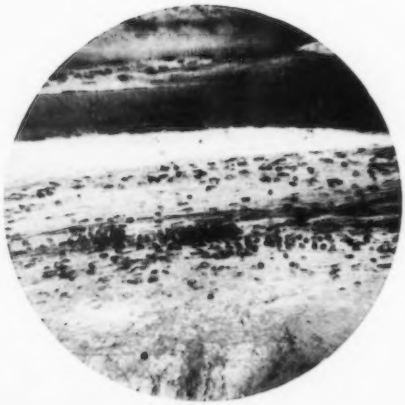

FIG. 5.

substance of the muscle fibres (fig. 5); these nuclei gradually increase in number till they completely fill the muscle fibre; then, after a short distance, they again become less numerous, and the muscle fibre again resumes its striation. The nuclei are rounded, have a diameter of about ·006 mm. (fig. 5), they stain well with hæmatoxylin, and generally occur about the equatorial region of the spindle.

In the equatorial region of the spindle the muscle fibres usually lie to one side, as they do not completely fill the space, whereas at the proximal and distal ends the sheath closely envelopes the muscle fibres. The muscle fibres, after passing through these changes in the equatorial region of the spindle, pass to the distal end, where they become joined again and pass out of the spindle. I do not believe that all fibres become so nucleated, for I have seen muscle fibres pass through a spindle without losing their striation at any point. The number of muscle fibres in a spindle varies according to the different regions seen in section.

Nerves and Nerve-endings.—The nerve supply is always abundant; as a rule, at least two nerves pass to a muscle-spindle, the one at the equatorial region, the other at the proximal or distal end. It is not uncommon, however, to find four nerves entering a spindle at various points, two or more arising from a common nerve trunk (figs. 1 and 10).

The largest nerve bundle usually enters the spindle at the equatorial region, and contains two or three fibres, the largest measuring about ·008 mm. The nerve bundles which enter toward the distal and proximal ends are generally composed of finer fibres, and sometimes of only a single fibre having a diameter of ·004 mm. or less.

These nerve fibres seem to terminate in various ways; as a rule, the large fibre which enters the equatorial region passes directly to the muscle fibre, and seems to lose itself in the nuclei of the muscle fibre above described (fig. 5); some fine fibres pass between the muscular fibres and terminate in such an organ as is figured in (fig. 7); others seem to have a spiral form. Others, again, form a fine plexus beneath or in the sheath of the spindle (fig. 6). So far

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PLATE III.

FIG. 6.

Normal Spindle in Longitudinal Section.

Stained by modified Pal's Method to show nerve plexus toward the polar region of the spindle.

Letter N points to position of nerve termination, which is seen magnified in fig. 7.

Magnified 28 diameters.

FIG. 7.

Same as fig. 6.

Under higher magnification to show nature of nerve ending within the spindle.

Diameter of terminal end bulb (N)=·008 mm.

Magnified 375 diameters.

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PLATE III.

FIG. 6.

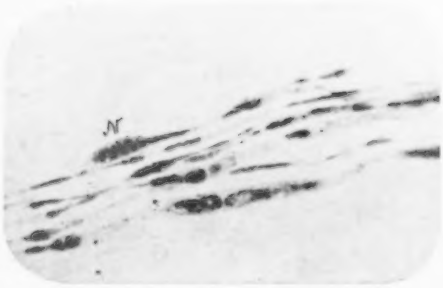

FIG. 7.

as my observations go, I have never seen a true motor end plate within a spindle. With regard to the number of nerve fibres within a spindle, one may see at times eight to ten in section. As a rule they are more numerous in the equatorial region than at the distal or proximal ends, but they are abundant in both situations.

Nerve Sheath.—Each nerve bundle passing to a spindle has a definite nerve sheath, composed of two or more layers. The nerve bundle passing to the equatorial region of the spindle has usually a sheath composed of eight or more laminæ, whilst the nerves entering at the distal or proximal ends have a sheath containing two to three layers. The sheath of the nerve passes directly into the sheath of the spindle as the nerve enters.

Spindle Sheath.—The sheath of the spindle resembles that of the nerve, and, as has been pointed out by certain authors, has the appearance of an onion in cross section. The laminæ are extremely fine, and possess elongated nuclei at intervals (fig. 14). At the equatorial region these laminæ are numerous, an average number being eight to ten; at the distal and proximal ends of the spindle the number of laminæ become less, and eventually pass into the muscle sheath. The characters of the sheath are best studied in cross section.

Blood Vessels.—The muscle-spindle is supplied with arteries and veins, which most frequently enter the spindle near the entrance of the central nerve, although vessels may be seen entering the spindle at various points (fig. 1).

Lymphatic and Lymphatic Spaces.—A lymph space is situated about the equatorial region of the spindle, and occupies about the middle one-third of the spindle. In some of my specimens the space exists as such, whilst in others it is filled up by a granular material, which does not stain readily. Sherrington has been able to inject this space by injecting the lymphatics of the leg.

Lymphatic vessels can also be seen in the teased specimens running parallel to the course of muscle fibres, and forming junctions with other lymphatics.

Septa.—Between the groups of muscle fibres and nerves within the spindle, septa are often seen dividing the spindle

PLATE IV.

FIG. 8.

Transverse Section from Tricep Muscle of Infantile Paralysis.

Two muscle spindles (S) are seen lying in completely atrophied areas.

Diameter of spindle { (i) = ·25 / (ii) = ·17

Magnified 24 diameters.

FIG. 9.

One of the above spindles under higher magnification, eight muscle fibres of varying sizes can be seen within the spindle, and two nerve fibres (N) which are well stained by Pal's method.

Size of nerve fibres	·01 and ·003
Size of muscle fibre	·024
Diameter of spindle	·25

Magnified 160 diameters.

XUM

XUM

PLATE IV.

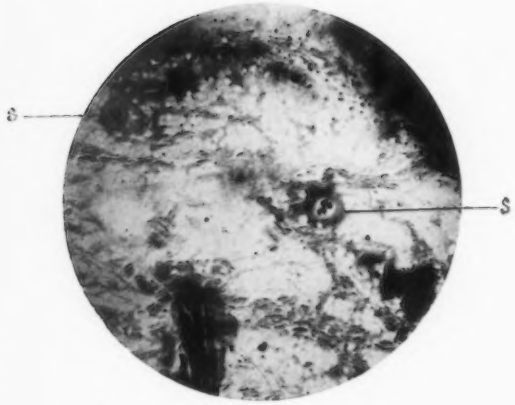

FIG. 8.

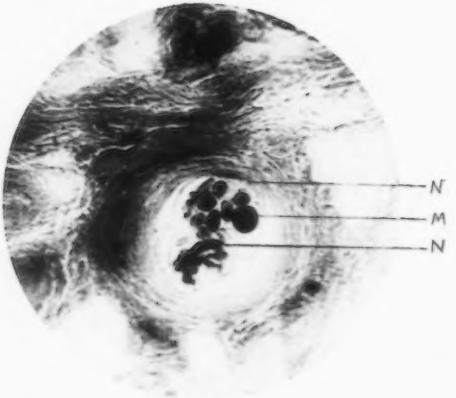

FIG. 9.

into two or more parts; these septa are extremely fine, and pass into the sheath of the spindle.

TABLE I.

Normal Muscle.

	Muscle.	Size of Extrafusal Muscle Fibres.	Size of Spindle (outside).	Size of Intrafusal Muscle Fibres.
Man.	Biceps.	·06 mm.	·16 mm.	·016.
Child.	Biceps.	·02 mm.	·16 mm.	·016.

Measurements in Millimeters.

(V.) THE CONDITION OF THE MUSCLE-SPINDLE UNDER THE FOLLOWING PATHOLOGICAL CONDITIONS.

(i.) Infantile paralysis; (ii.) tabes; (iii.) myopathy (Leyden's form); (iv.) progressive muscular atrophy; (v.) peripheral neuritis; (vi.) injury to brachial plexus; (vii.) after section of sciatic in cats.

I do not propose to enter into the clinical history of these various cases, or to describe the pathological changes in the various muscles under examination, except in so far as they concern the muscle-spindle.

(i.) *Infantile Paralysis.*—I have been able to examine the muscle from six cases of infantile paralysis; five of these were from museum specimens, and, having been preserved in spirit, could not be stained by Pal's method. In the sixth case I was able to obtain the arm fresh, and have been able to treat it by the methods above described. In all the cases the paralysis was of long standing. In four of the cases, thirteen muscles were examined; all these muscles were considerably atrophied; some had apparently no normal muscle fibres left, yet in all of these the muscle-spindles could be demonstrated, and the intrafusal fibres were about the usual size (fig. 8). In the other two cases

the whole substance of the muscle was replaced by fat, and in these two specimens I was unable to find any spindles (only small pieces of these muscles were obtainable). What is the condition of the nerves entering the muscle-spindle? In answer to this question I was able to examine the muscles of case No. 6 by Pal's method, and it is found that not only the nerve entering the equatorial region (fig. 9) of the spindle, but also the plexus of nerves towards the polar ends, remain intact. The teased specimens give the same result, and also show that the striation of the intrafusal fibres is well preserved. In regard to infantile paralysis, it would seem probable that the muscle-spindle remains absolutely intact, both in regard to the intrafusal muscle fibres and in regard to the contained nerves. Table II. gives details of measurements and muscle examined.

(ii.) *Tabes.*—I have examined the muscles from three cases of tabes. In two of these I had only portions of the muscle, which were given to me after being in Müller's fluid for some time. These I examined by the usual methods, staining them with hæmatoxylin and by Pal's method, and, so far as the examination goes, it shows that the spindles are normal, both in regard to the intrafusal muscle fibres and the nerve supply. With regard to the third case, I have been able to examine it with greater care. Spindles have been teased out in the usual manner from the rectus femoris, vastus internus, and biceps of the arm. So far as the shape of the spindle and the nerves entering it is concerned, no change can be seen (fig. 10). The spindle does not, however, stain so readily with hæmatoxylin as does the normal spindle, and especially the equatorial region, which, as a rule, stains very deeply. The intrafusal nerve fibres, however, stain well. The striation of the intrafusal muscle fibre is well preserved. Examining the specimens of a spindle cut in longitudinal section, stained by Marchi solution, one finds, situated in the muscular fibre in the same position as the round cells which have been above described, evidence of degeneration (fig. 11). If now one examines the section stained by Pal's method, in order to find out if any degeneration has occurred in the nerve fibre passing to this

TABLE II.

Infantile Paralysis.

Case.	Muscle.	Condition of Muscle.	Size of Extrafusal Muscle Fibres.	Size of Spindle outside.	Size of Intrafusal Muscle Fibres.	Nerve Staining and Size within Spindle.
I. Man	Triceps	Extensive atrophy	·04 mm.	·12 mm.	·018	—
II. Child	Biceps femoris	Atrophied	·008	·1	·016	—
	Semi-membranous	Atrophied	·006	·12	·016	—
	Rectus femoris	Atrophied	·008	—	·015	—
III.	Anterior tibial	Some normal fibres	·07	·112	·013	—
	Gastrocnemius	Irregular atrophy	·014	·072	·011	—
	Permei	Irregular atrophy	·03	·056	·011	—
	Solens	Well developed	·057	·12	·013	—
IV.	Gastrocnemius	Fatty, with scattered fibres	·08	—	—	—
V.	Gastrocnemius	Fatty	—	—	—	—
VI. Woman	Deltoid	Partly atrophied	·04	·088	·015	Nerve well stained ·004
	Biceps	Partly atrophied	·035	·132	·013	Nerve well stained
	Supinator	Partly atrophied	·073	·14	·014	Nerve well stained
	Triceps	Completely atrophied	—	·168	·018	Nerve { ·008 mm. ·003 mm.
	Abductor pollicis	Completely atrophied	—	·18	·012	Nerve well stained

PLATE V.

Fig. 10.

Normal Spindle from Vastus Internus (Tabes).

Teased Specimen. Sihler's Method.

Length	11·9 mm.
Breadth	·51 mm.

Nerve bundles are seen entering at four points, 1, 2, 3, 4.

No. 1. contains the largest nerve fibre measuring ·008 mm. and terminates about the equatorial region of the spindle.

The other nerves entering towards the poles are finer, varying from ·003 to ·006 mm.

At the point marked 2 the nerve divides, one part can be seen passing to the motor nerve ending on the muscle fibres extrinsic to the spindle, the other enters the spindle.

Magnified 7 diameters.

Fig. 11.

Spindle from the Rectus Femoris (Tabes).

Cut in longitudinal sections to show degenerative change within the intrafusal muscle fibre. Stained in Marchi's solution.

The degeneration is manifest by the deposit of fat in that portion of the intrafusal muscle fibre when it has been shown that intrafusal muscle nuclei exist, the remaining portion of the muscle fibre remaining free.

Ms	Extrafusal muscle fibre.
M	Intrafusal muscle fibre.
Sh	Sheath.
D	Area of degeneration.

Breadth of spindle	·18
Breadth of muscle fibre	·024

Magnified 150 diameters.

Fig. 12.

Spindle from Rectus Femoris (Tabes).

Cut in longitudinal section stained by Marchi-Pal Method to show that the nerve passing to the degenerate area within the spindle remains normal. The degenerated area within the muscle-fibre is apparent.

Intrafusal muscle fibre	·028

Magnified 180 diameters.

XUM

XUM

PLATE V.

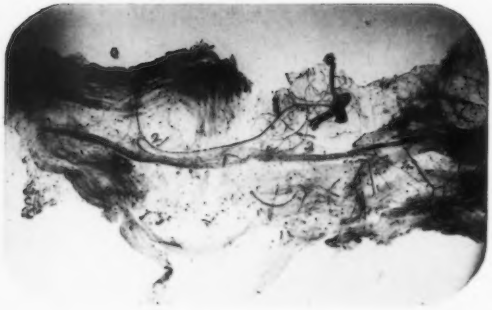

FIG. 10.

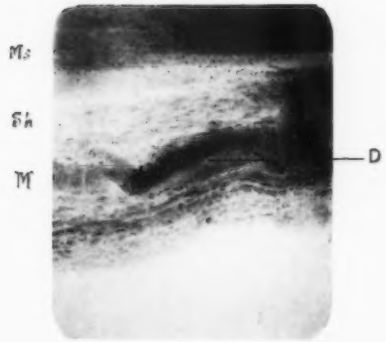

FIG. 11.

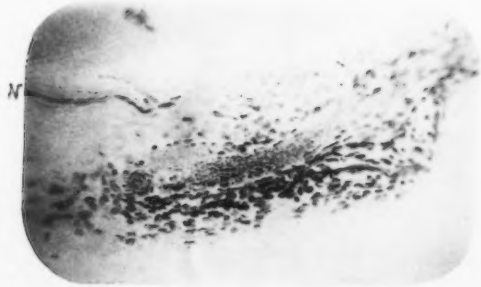

FIG. 12.

spot, one finds that the nerve fibre stains well, and shows no sign of degeneration (fig. 12). In a third specimen the same appearance is observable. It is difficult to prove that the above-described condition is truly pathological, but it is a condition which I have found in tabes, and not under any other circumstances. That changes should be found in the terminal organs, while the nerve lying between the ganglion cell and the terminal organ remains intact, seems to me not only a possible, but a probable lesion, for it has been shown by Cattaneo (5) that, after section of the whole nerve, changes take place in the nerve termination within thirty-six to thirty-eight hours, while changes in the medullated portion of the nerve are extremely slow. So that in tabes, where one has a gradual degeneration taking place in the cells of the posterior ganglion, it is not unreasonable to suppose that changes take place at the extreme end of the neuron, without any change being manifest in the nerve itself. With regard to the other nerve-endings within the spindle, I find no evidence of degeneration (see Tables III. and IV.).

(iii.) *Myopathy* (Leyden form).—I have had the opportunity of examining the muscles from one case of myopathy, and in this case the following muscles were examined:—Biceps (arm), pectoral major, pectoral minor, seratus, quadriceps, gluteus, and rectus abdominis. The sections have been prepared in the usual manner, except that there are no teased specimens. With regard to the condition of the spindle, it is remarkable, on examining the biceps, how numerous the spindles seem to be, as many as seventeen being found cut in one section. I have never found so many in any normal biceps, not even in the biceps from a boy of the same age, who died of phthisis. It was such an appearance which gave rise to the idea that these spindles were pathological bodies occurring in wasting muscles. This apparent increase is not so noticeable in the other muscles examined. With regard to the nerve supply, it is found that the nerves to the spindle all stain well by Pal's method, and the arrangement of the nerve fibres can be well studied. The striation of the muscle fibre is well marked within the spindle. In many of the spindles the intrafusal

TABLE III.

Tabes.

Case.	Muscle.	Condition of Muscle.	Size of Extrafusal Muscle Fibre.	Size of Spindle outside.	Size of Intrafusal Muscle Fibre.	Nerve Staining within Spindle.
I. Man	Vastus Internus (left)	Normal	·048	·072	·015	Well stained
	Vastus Internus (right)	Normal	·048	—	—	—
II. Man	Vastus Internus	Normal	·06	—	—	—
III. Woman	Rectus femoris	Normal	·04	.1	·012	Well stained
	Vastus Internus	Normal	·04	·24	·018	Well stained
	Biceps	Normal	·036	·15	·02	Well stained

TABLE IV.

Tabes (Teased Specimens).

Case.	Muscle.	Length of Spindle.	Breadth.	Number of Nerves entering Spindle.	Size of Intrafusal Muscle Fibre.	Nerves.
III. Woman	Rectus femoris	—	—	3	·04	Well shown
	Vastus internus	11·9 mm.	·51 mm.*	3	·02	Well shown
	Biceps	8·5	·5 mm.	4	·028	Well shown

* Probably increased by pressure of cover glass.

fibre is as large, and often larger, than the extrafusal fibre, the same condition is met with in the muscles of young children. In myopathy, then, the muscle-spindle remains without alteration (see Table V.), and (fig. 13 and 14).

(iv.) *Progressive Muscular Atrophy.*—The condition of the muscle-spindle in progressive muscular atrophy has been worked at by more than one author, viz., Pilliet, Blocq and Marinesco and Forster, and they agree that the spindle does not undergo alteration. Forster has further shown that the nerve to the spindle remains intact. I have had the opportunity of examining muscles from three cases of progressive muscular atrophy. In the teased specimens from these cases it is noticeable how the large nerve fibre passing to the spindle stands out against the surrounding atrophied fibres in the nerve. The spindle itself remains unaltered, the nerve fibres within the spindle are natural, and the intrafusal fibres preserve their striation. My observations, then, would agree with the above authors, viz., that the muscle-spindle remains unaltered in progressive muscular atrophy (fig. 16 and Table VI.).

(v.) *Peripheral Neuritis.*—I have had the opportunity of examining the muscles from only one case of peripheral neuritis, and in this case the atrophy of the muscles examined is not extensive; therefore it is a case of no great value in regard to the condition of the spindle. Three muscles were examined—the extensors of the wrist, the supinator longus, and the extensor longus digitorum. On microscopical examination of the first two named muscles, very little change is noted in the muscular structure. In the extensor longus digitorum there is a considerable amount of fat, some fibres considerably atrophied, others normal in appearance and size. In the extensor longus digitorum a muscle-spindle is seen in the middle of a completely atrophied area, the walls and muscle fibres appear to be normal, sections from Marchi's solution show no change, and staining with Pal's method shows the nerve fibres are normal. The other muscles stained in a similar manner also show the normal spindle. In the teased specimens the intrafusal muscle fibres preserve their striation, the extrafusal muscle

PLATE VI.

FIG. 13.

Transverse Section of Biceps from a Case of Myopathy.

To show the prominence given to the muscle spindle by atrophy of the muscle—three spindles in transverse section are seen.

Diameter of spindle .. .. ·18 mm.

Magnified 44 diameters.

FIG. 14.

One of the above spindles under a higher power. The sheath is well shown, the intrinsic muscle fibres are equal in size to the extrinsic (compare fig. 4). The varying size of the intrafusal muscle fibres is well shown.

Diameter of spindle	·18
Intrafusal muscle fibres	·024

Magnified 180 diameters.

XUM

XUM

PLATE VI.

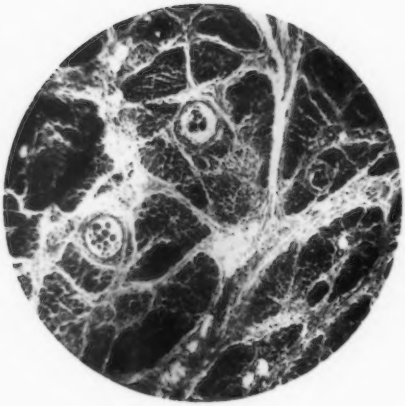

FIG. 13.

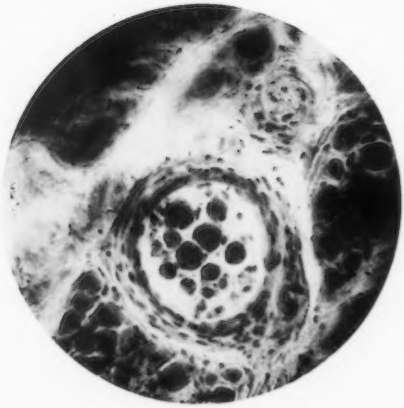

FIG. 14.

XIIM

TABLE V.

Myopathy.

Boy aged 5.

Muscle.	Condition of Muscle.	Size of Extrafusal Muscle Fibres.	Size of Spindle.	Size of Intrafusal Muscle Fibres.	Nerve Staining within Spindle.
Biceps	Atrophied	·024 ·008 ·048	·180 mm	·032	Nerve well stained
Pectoralis major	Atrophied	·04	·160 mm	·02	Nerve well stained
Serratus	Atrophied	·054	·12 mm	·024	—
Pectoralis minor	Atrophied	·036	—	—	—
Quadriceps	Atrophied	·024	·1 mm	·024	Nerve well stained
Gluteus	Very fatty	·016	—	—	—
Rectus abdominis	Normal	·04	—	—	—

TABLE VI.

Progressive Muscular Atrophy.

Case.	Muscle.	Condition of Muscle.	Size of Extrafusal Muscle Fibre.	Size of Spindle.	Size of Intrafusal Muscle Fibre.	Nerve Staining.
I. Man	Biceps	Atrophied	·044	·14	·02	Well stained
II. Man	Abductor minimi digiti	Atrophied	·04-·008	·09	·02	Stained
III. Man	Abductor minimi digiti	Atrophied	·012	·14	·016	Well stained
	Flexor brevis	Atrophied	·012	·16	·024	Feebly stained

XUM

PLATE VII.

FIG. 15.

Compound Spindle from Biceps of Myopathy.

Seen in Transverse Section. Stained by Pal's Method.

This is probably an instance of a quadruple spindle, it is noticeable that the spindles are cut in varying parts of their course. Six divisions are seen, two of these (NN) contain nerve fibres as shown by the staining method, the other four contain muscle fibres. It is obvious that the poles of these spindles cannot lie on the same plane.

Diameter	·34 mm.
Nerve fibre	·004 mm.

Magnified 47 diameters.

XUM

PLATE VII.

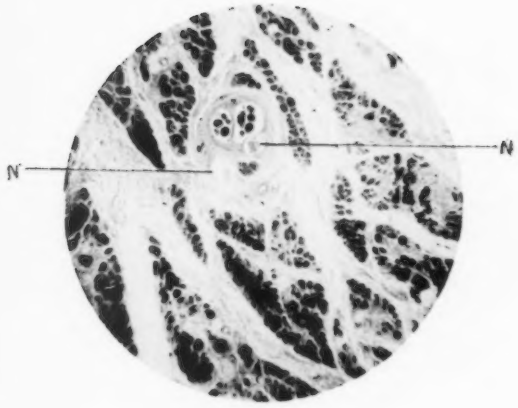

FIG. 15.

XIIM

fibres have, in many fibres, lost their striation, and have a granular appearance. So far as one may judge from this case, one may say that in peripheral neuritis the spindle and its nerve remain intact after the muscle has become extensively atrophied; this is not, however, in agreement with Gudden's (16) statement (Table VII.).

(vi.) *Injury to the Brachial Plexus, with Complete Loss of Motion and Sensations of a year's standing.*—In this case, I have examined the biceps, triceps, muscles of the thumb, muscles of the little fingers, flexors and extensors of the wrist, and the ulnar and median nerves. All these muscles show most extensive atrophy; in many there are no recognisable fibres present, but in the biceps and triceps a few remain.

The ulnar and median nerves both show a very few fibres stained by Pal's method, which appear normal (these fibres measure about ·004 mm., and one or two ·008 mm.).

What is the condition of the muscle-spindle under these conditions? The spindle can be recognised in most muscles on careful examination, but it does not stand out in contrast to the surrounding tissue; all the spindles are small, the intrafusal muscle fibres atrophied, and the nerve fibres to the spindle do not stain by Pal's method. (There is one exception in a section of the triceps, one nerve fibre having taken the stain.)

In this case I have had considerable difficulty in teasing out the muscle-spindle; this I attribute partly to the fibrous increase in the muscle, and partly to the atrophy of the spindle. In the spindles which I have, however, teased out, the nerve is atrophied, the shape of the intrafusal muscle fibres can still be seen, but the transverse striation is almost completely lost and the fibres have a granular appearance; the usually darkly-stained portion in the equatorial region is absent, pointing to degeneration occurring in the intramuscular nuclei in this region. The spindle sheath can still be distinguished, but is not so distinct as normally. (Fig. 17).

It would seem, therefore, from the examination of the above, that atrophy of the spindle and its contained muscle fibres does take place, but at a much later period than the ordinary muscle fibre.

XUM

PLATE VIII.

FIG. 16.

Muscle Spindle from Hypothenar Muscles of Progressive Muscular Atrophy.

To show unaltered condition of muscle spindle in this disease.

The intrinsic muscle fibres retain their normal size and striation, while the surrounding muscle fibres have undergone extensive atrophy, being reduced to $\frac{1}{10}$ of their normal size.

Diameter of spindle .. .. ·14 mm.

Magnified 180 diameters.

FIG. 17.

Muscle Spindle from Hypothenar Muscles of a Case of Injury to Brachial Plexus.

Transverse section.

Two spindles are seen in transverse section lying in areas where extensive atrophy has taken place. The muscle fibres are smaller than normal, and appear shrunken.

Magnified 120 diameters.

XUM

PLATE VIII.

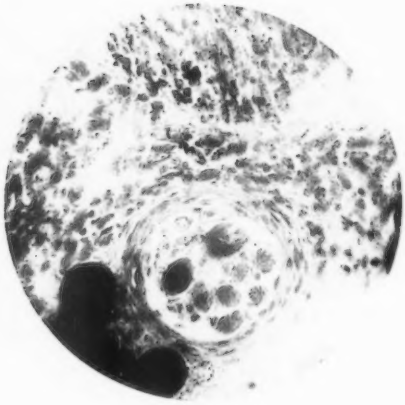

FIG. 16.

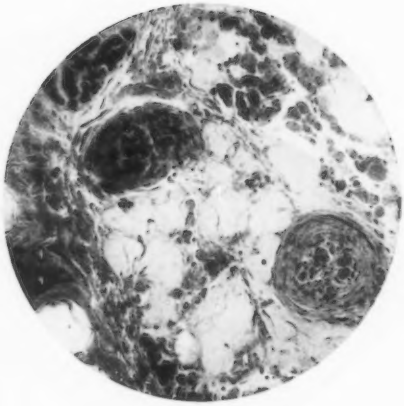

FIG. 17.

TABLE VII.

Peripheral Neuritis.

Case, Woman.

Muscle.	Condition of Muscle.	Size of Extrafusal Muscle Fibre.	Size of Spindle.	Size of Intrafusal Muscle Fibre.	Nerve Staining.
Extensors of wrist	Not much atrophied	·048	·2	·02	Well stained
Supinator longus	Irregular in size	·08—·02	·2	·016	Well stained
Extensor longus digitorum (leg)	Atrophied	·1—·008	·18	·02	Well stained

TABLE VIII.

Injury to Brachial Plexus.

Muscle.	Condition of Muscle.	Size of Extrafusal Muscle Fibre.	Size of Spindle.	Size of Intrafusal Muscle Fibre.	Nerve Staining.
Biceps	Atrophied	·06—·004 mm	·072	·02	Absent
Triceps	Atrophied	·048—·004	·08	·02	Present (one fibre)
Thumb muscles	Atrophied	·02—·004	·08	·012	Absent
Hypothenar	Atrophied	·02—·004	·16	·012	Absent
Extensors of wrist	Atrophied	·02—·004	·052	·008	Absent
Flexors of wrist	Atrophied	·02—·004	·01	·016	Absent

XUM

(xii.) *Section of Sciatic in Cats*[1]—three weeks, one month, two months, three months.—I have (thanks to the kindness of Dr. Risien Russell) had the opportunity of examining the gastrocnemius of four cats, at various times after complete section of the sciatic. The gastrocnemius of the cat is composed of two parts, one being white muscle fibres and the other red. I have examined muscles at the following times, after section of the sciatic: three weeks, one month, two months, three months, and have also examined

TABLE IX.

Cats—Section of Sciatic.

Muscle Gastrocnemius.

Cat.	Red Muscle.	White Muscle.	Size of Spindle outside.	Size of Spindle inside.	Intrafusal Muscle Fibre.	No. of Fibres in Spindle.
Normal	·052	·047	·12	·08	·0216	5
Cat I. (3 weeks)	·0412	·036	—	—	—	—
Cat II. (1 month)	·0505	·034	·16	·08	·018	7
Cat III. (2 months)	·0256	·0228	·1	·064	·0143	7
			·096	·056	·019	5
Cat IV. (3 months)	·0184	·0152	·048	·032	·019	7

the normal muscle for comparison. In the normal muscle, the extrafusal fibres measure between ·04 to ·06 mm. The white fibres are somewhat smaller than the red. The intrafusal fibres measure about ·02 mm. Muscle-spindles occur in both forms of muscle, and to an equal extent, so far as I can judge. Table No. IX. shows the comparative size of the red and white muscle fibres at the various periods after section of the nerve, and also the size of the intrafusal muscle fibres. The

[1] From Experiments performed at University College Laboratory.

table tends to show that atrophy takes place rather more rapidly in the white muscle fibres than in the red. I cannot say there is any definite atrophy in the intrafusal fibres. Sections stained by Pal's method show that the nerve to the spindle is degenerate in all cases. Striation is certainly well-marked in the intrafusal fibre three months after section of the nerve, and Sherrington has shown that striation still persists 150 days after section of the nerve. I believe, however (as the case above related tends to prove), that degeneration does take place in the intrafusal muscle fibres if sufficient time is allowed to elapse.

Summary.

Historical.—Although certain observers, fifty years ago, seem to have described in muscle, bodies which we now recognise as muscle-spindles, Kölliker was, however, the first who gave a detailed description of these bodies, and soon afterwards Kühne added considerably to the description of Kölliker, and introduced the name "muscle-spindle"; both these authors regarded the spindle as a stage in the development of muscle. Then followed a series of authors who regarded the spindle as a pathological condition occurring in atrophied muscle, and even as late as 1890 one author still regards them as pathological. Sachs, in 1874, fully recognised the sensory termination in muscle, but it was not till 1888 that Kerschner argued this sensory nature of the muscle-spindle, and suggested that their function might be connected with the sense of position. In the same year Cattaneo suggested that the muscle-tendon organs described by Golgi had a similar function, and proved experimentally that these bodies were connected with the posterior nerve roots. Then follow a series of authors working with normal and pathological material, some of whom adopted Keschner's view, others again adopting the view of Kölliker. Sherrington definitely proved, by degenerative experiments, that the nerve fibre from the muscle-spindle passes up in the posterior nerve root. All the later evidence points to the sensory

XUM

nature of the muscle-spindle. The muscle-spindle has been shown to be a spindle-shaped body, composed of a sheath resembling the Henle sheath of a nerve; within this sheath are contained small muscle fibres, which at one point become densely nucleated, and lose their striation. Two or more nerve bundles enter the muscle-spindle, and terminate —(i.) in the muscle fibres, (ii.) between the muscle fibres, and (iii.) in the sheath. The spindle also contains blood-vessels and lymphatics. These spindles are found in nearly all muscles of the body, except the eye muscles, the intrinsic muscles of the tongue, and the diaphragm. It has been shown, in the present paper, that in infantile paralysis the spindle remains absolutely normal, although the surrounding muscle tissue undergoes complete atrophy. In tabes it is shown that certain changes take place in the termination of the nerves, the general structure of the spindle remaining normal. In myopathy, the spindle and its contained nerve are normal. In progressive muscular atrophy the spindle remains unaltered, and the same is probably true with regard to peripheral neuritis. Section or injury of nerve trunk leads to atrophy of the muscle fibre within the spindle, though it is probable that it takes a considerable length of time for changes to take place in the muscle fibre within the spindle.

In conclusion, I would wish to take this opportunity of thanking the pathologists of the National Hospital, Dr. Colman and Dr. Risien Russell, for the kind manner in which they have assisted me in my work. To many others I would also tender my thanks for having allowed me to make use of material which I needed for my work, and especially to Dr. Barlow, Mr. Willett, and Dr. Kanthack.

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ON A METHOD OF DEMONSTRATING SECONDARY DEGENERATIONS OF THE NERVOUS SYSTEM BY MEANS OF PEROSMIC ACID.

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ALL who have had practical experience of Marchi's perosmic acid method of demonstrating secondary degenerations of the central nervous system, must have found that there are certain drawbacks to its use which render it almost completely inapplicable for entire brains or large masses of the same. When it succeeds, no doubt, the result is highly to be commended. The clearness with which the oil globules can be shown in the degenerated tracts is beyond all praise.

The reaction is always most successful in small pieces of tissue, such as short segments of the spinal cord, a nerve trunk, or a piece of brain substance of cubic capacity not greater than one or two centimètres. Even in the case of a large spinal cord, such as that of man, only a limited length, amounting to something like a couple of centimètres, can be stained successfully by means of it. When larger pieces of tissue are immersed in the solution, the perosmic acid often fails to penetrate to the interior. So great are the hardening properties of the solution, and so rapidly does it act, that the cord or other piece of nerve tissue placed in it is apt to be, so to speak, "case hardened." The outside becomes quite horny in consistence, and perfectly black, while the interior remains uninfluenced. When this has happened, no amount of renewal of the solution or increase of its strength will cause the perosmic acid to penetrate

further. Indeed, with the use of these expedients the tendency is rather to increase than to decrease the evil. The exterior becomes so brittle that it is found to be impossible to cut sections, even after the most careful imbedding in celloidin. In the case of an entire brain, even of a small animal like a cat or monkey, no reliance can be placed on the solution penetrating to the interior. The stain of the surroundings, moreover, is apt to be too dark, so that there is difficulty in seeing the oil globules. Then the quantity of perosmic acid necessary is large, and involves very considerable outlay. With experimental brains these difficulties are almost insuperable, and it was after the bitter experience gained by a succession of failures in such cases that I resolved to start an inquiry, now many months ago, with the view of finding out whether there was not some means of applying the perosmic acid more to be relied upon than that comprised in this otherwise estimable method.

Did the degenerated tissue stain with Marchi's liquid when cut into sections, the difficulties would be in great part overcome, but one of the most evident facts connected with the process is that neither by the ordinary method nor by any modification of it is it possible to call forth the reaction after the sections have once been cut. For months I worked at every conceivable means whereby this much-to-be-desired result might be attained, but in all instances with signal failure. A spinal cord, for instance, which gave an exquisite reaction in the degenerated tracts when stained *en bloc*, utterly failed to show a single oil globule when treated in section by the reagent.

It was only lately that I discovered what appears to be the explanation of this most remarkable phenomenon. When the Müller's fluid is entirely removed from such a cord by prolonged soaking in running water, the reaction with Marchi's solution is no longer forthcoming. A piece of the above cord, which gave a brilliant reaction when transferred to the Marchi's solution directly from the Müller's fluid in which it had been hardened, failed to do so when the Müller's fluid had been removed from it.

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It struck me, therefore, that there must be something contained in the tissue hardened in Müller's fluid which is soluble in water, and which is the means of fixing the perosmic acid upon the globules of oil. If this theory be correct, the substance contained in the hardened tissue ought, when separated, to have the same properties. The substance in question may probably be soluble also in spirit, ether, &c., and the cause of the section not staining with the Marchi's reagent may be that it is washed out in the process of imbedding, cutting, &c. The substance must be a product formed slowly in the hardening of the part, for it is those tissues which have been longest immersed in the Müller's fluid which give Marchi's reaction best.

Acting upon this observation, I pounded up in a mortar a piece of brain which had been for many months in Müller's fluid, with some of the Müller's fluid in which it had been hardened, and filtered off the liquid after reducing the mass to a thin cream-like consistence. The liquid which came through the filter was perfectly transparent, and, with the exception of being a little darker, did not differ from ordinary Müller's fluid. Two parts of this liquid were mixed with one part of a 1 to 100 solution of perosmic acid, as in the making of Marchi's reagent. To my delight, after I had kept sections of the cord above referred to in the mixture at a body temperature for twenty-four hours, a decided reaction was visible in the oil globules in the degenerated parts. So decided was it that even in unclarified preparations the globules could be readily enough detected. The reaction, however, was not so good as when brought about by Marchi's method. Neither were the blackened oil globules so numerous, nor were they so black as when the parts were treated by the ordinary means. Still there was evidently something contained in the solution which had the property of fixing the perosmic acid upon the oil globules in a manner which I had not been able to obtain by any previous means.

It struck me that, by placing the sections, after being stained in the solution, in a reducing liquid, the blackening of the oil globules might be much intensified. I used

for this purpose a mixture of pyrogallic acid, glycerine, spirit, and water, but found that when the preparations had been in this at a body temperature for twenty-four hours they were so intensely black all over that the oil globules were lost in the universally black background. On treating the sections, however, with solution of permanganate of potash, so as again to oxidize the reduced perosmic acid, and subsequently decolorizing in sulphurous acid, the blackening left the surroundings and remained upon the oil globules.[1]

The latter stood out in a manner comparable to the best results obtained by Marchi's method.

The solution of perosmic acid, however, seemed to me too strong. It took too long to remove the stain from the surroundings, and I, accordingly, went on diluting it, and studying the results. As the sum of the various trials made, I found that a mixture of one part of a 1 to 100 solution of perosmic acid in distilled water to *two hundred* of the filtered brain liquid is best.

Employing such a diluted solution of perosmic acid (1 to 20,000), the degenerated fibres do not blacken until acted upon by the reducing agent. The section, when taken out of the liquid, has a universal brown colour, without any black points being visible upon it. But in some unaccountable manner the liquid has the property of fixing the perosmic acid upon them more than upon the surroundings.

In order to test the staining powers of this brain liquid solution of perosmic acid as compared with other solutions of perosmic acid, I cut a number of sections of a human spinal cord secondarily degenerated after six weeks' com-

[1] Heller, I found, had also used permanganate for the purpose of removing the reduced perosmic acid from the surroundings of stained, medullated, healthy nerve fibres. He employs oxalic acid for the purpose of destroying the brown colour imparted by it to the tissue. I prefer sulphurous acid. My own observations on the employment of a reducing agent followed by permanganate were made independently of, and before his were published. The sulphurous acid removes the colour imparted to the tissue by the permanganate in virtue of its strong affinity for oxygen, converting, as it does, the permanganic acid into the colourless protoxide of manganese. A ferrous salt has the same action.

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pression, and placed some of them in each of the following solutions: (*a*) Brain liquid 200 parts, and perosmic acid solution in distilled water (1 to 100) 1 part; (*b*) Müller's fluid 200 parts, and perosmic acid solution in distilled water (1 to 100) 1 part; and (*c*) distilled water 200 parts, and perosmic acid solution in distilled water (1 to 100) 1 part. They were all treated subsequently in exactly the same way. In the case of *a* the staining of the degenerated tracts was intense; in that of *b* only a fibre here and there was stained; and in that of *c* the degenerated tracts were practically unstained.

On comparing sections of this cord stained by Marchi's method with those of the same stained by my own, I was struck by the fact that the reaction in the latter was more delicate than in the former. Long before oil globules had made their appearance in the degenerated fibres, at the time when the myeline sheath was breaking up and giving rise to a varicose aspect of the fibre, a decided reaction was obtained. In a case of ascending degeneration of the cord, for instance, over and above the intense blackening in the recognised ascending tracts, odd fibres, having a disintegrated appearance, were found scattered over the section, which gave an intense black reaction. These were only faintly touched by Marchi's method, and many of them were not indicated at all. What was also most striking in this same cord was that the crossed pyramidal tracts in the part above the point of compression were devoid of any blackened fibres, and the fibres within them were seen to have a perfectly normal aspect. Axis-cylinder and myeline sheath were each undisturbed.

It might be asked, however, what is the effect upon a normal cord or brain? Does the black stain not remain upon the normal nerve fibres? In order to test this, I used a human medulla oblongata from an adult, the brain of a kitten, the cords from a monkey and rabbit, and the brain, cord, and nerves from a hedgehog. They were all hardened in Müller's fluid and treated as described subsequently in detail. In none of them, after being the prescribed length of time in the permanganate of potash, could I find a single

nerve fibre which had given the black reaction. A black precipitate occasionally formed here and there, probably some compound of the iron of the blood with the pyrogallic acid, but the colour of the section, otherwise, was a universal greenish-grey. The irregular varicose aspect of those fibres which stained black in degenerated cords, together with the breaking up of the myeline sheath, was additional proof of their being abnormal.

Such are briefly the outlines of the method. Its success seems to depend upon something being contained in the brain hardened in Müller's fluid, which is soluble in various media, and which has the property of fixing the perosmic acid upon the fattily degenerated parts more than upon those which are normal. When, consequently, the reduced perosmic acid is oxidised by the permanganate, the fattily degenerated parts lose their colour less readily than the surroundings.

After many trials and modifications of the method, I find the following procedure gives the best results:—

The parts must be hardened in Müller's fluid. The human brain, or brains of large animals, should be *injected* with the same, after the manner described in my "Text-book of Pathology," vol. i., p. 56. From three to four months' time is requisite to complete the hardening. The pieces of tissue to be cut, without being washed, are dehydrated and imbedded in celloidin. It is a remarkable fact that, even when they are thoroughly dehydrated with absolute alcohol, subsequently treated with ether and alcohol, and left, it may be for months, in an ethereal solution of celloidin, the oil globules are not washed out. Indeed, the staining capacity of the part does not seem to be altered in any way.

As the sections are cut they are placed in a capsule containing dilute alcohol, one above the other in serial order, and, if necessary, separated by pieces of thin paper.

They are next enclosed in a double film of collodion. Some thin collodion is poured over a slide and allowed to become firm on the surface, not too dry. The slide is then immersed in methylated spirit in a flat dish, and a section,

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or several sections, as the case may be, are placed upon it. A *printed number* of small size, corresponding to the place of the section in the series, is then fixed on the film of collodion close to the margin of the section. The spirit is dried from the slide by covering it with a piece of white tissue paper and pressing over this with a sheet of blotting paper. The tissue paper prevents the fibre of the blotting paper from adhering to the collodion. A second layer of collodion is now poured over the surface so as to encapsule the section and its attached number. When this again has become firm, not too dry, the slide is plunged for a few seconds or longer into methylated spirit, and the film of collodion with the section included stripped off. The margins are next clipped so as to leave a border of about 1 cm. all round, inclusive, of course, of the printed number. The sections can be kept permanently in pure methylated spirit or other form of dilute alcohol.

A piece of brain which has become quite brown by being kept in Müller's fluid for many months, it may be years, is pounded up in a mortar, and converted into a thin cream-like consistence by the addition of Müller's fluid. The Müller's fluid in which the brain employed has been hardened is best, but ordinary Müller does well enough. About half a kilo-weight of brain to the litre of Müller is sufficient. When the two are thoroughly incorporated, the liquid is filtered off through paper, and of this 200 cc. are mixed with 1 cc. of a 1 to 100 solution of perosmic acid in distilled water. The mixture is placed in a wide-mouthed stopper bottle, and the stopper is anointed with glycerine to render it air-tight. If necessary, the stopper should be tied down to prevent it becoming displaced. The incapsuled sections are immersed in a considerable excess of the reagent, and the bottle is kept in an incubator at a body temperature. The use of the incubator seems to be essential for success. They are left in the solution for twenty-four hours, and thereafter are taken out and washed for a few seconds in water. The bottle must next be thoroughly cleansed, and sufficient of the following reducing fluid introduced to cover the sections:—

Pyrogallic acid	1 grm.
Glycerine...	50 cc.
Rectified spirit or absolute alcohol	50 cc.
Water to...	400 cc.

The stopper is anointed with glycerine, replaced in the bottle, and the bottle put into the incubating chamber for twenty-four hours.

The sections and the bottle are again thoroughly washed in water. Particular attention must be given to seeing that this is done effectually. If any of the reducing fluid remains, the permanganate solution becomes decomposed and precipitated. Indeed the sections should be left overnight in water. The bottle is now filled with $\frac{1}{4}$ per cent. permanganate of potash solution, and the sections are immersed in this. The stopper is replaced, but the bottle is *not* put in the incubator. It is left at an ordinary sitting-room temperature. After a day's immersion, the sections are taken out, washed in water, and treated with sulphurous acid. The strength of the sulphurous acid does not make much difference; that of the British Pharmacopœia acts very quickly. Weaker solutions, however, may be employed, a longer time being allowed for decoloration. When thoroughly decolorised, the sections are again washed, and placed for twenty-four hours in the permanganate solution, and so on for three days, the decoloration with sulphurous acid being practised after every twenty-four hours—that is to say, three times in all. After the three days' treatment with permanganate and sulphurous acid, they are again subjected to the reducing fluid in the incubator for twenty-four hours. The object of this last proceeding is two-fold. The decoloration of the healthy parts of the section by the permanganate is so complete that, when clarified, they become almost invisible. The immersion in the reducing liquid over-night gives them a greenish-grey colour, and so renders them apparent. A second effect is that the degenerated fibres, blackened by the process up to this point, are rendered literally jet-black by the night's immersion in the reducing fluid.

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The last part of the procedure is to wash thoroughly in water, to dehydrate by immersing in several relays of dehydrated spirit followed by absolute alcohol, to clarify in oil of cloves or carbol-xylol, and to mount in dammar lac. The dehydration must be perfect, and to this end it is necessary to place the sections in excess of alcohol. For the first dehydration I use ordinary dehydrated methylated spirit, pouring an ounce or so into a capsule and immersing the sections in it for five to ten minutes. They are afterwards placed on a slide, and the excess of spirit removed by application of tissue and blotting paper as before described. Thereafter absolute alcohol is poured twice over the section, and allowed to remain in contact with it for a few minutes. The excess is removed from the margins with a soft cloth, not by using blotting paper. The most satisfactory method of clarifying is to plunge the slide, with the section upon it, into the clarifying agent, and to leave it in this until clarification is completed. The superfluity of the clarifying medium is removed with a soft cloth, and the section mounted in dammar lac.

The oil globules blackened by Marchi's method are liable to fade after being mounted for some time. So far as I have yet seen, after several months, those stained by my own method, if thoroughly washed after use of the sulphurous acid, are not.

The infinitesimally small quantity of perosmic acid necessary for my process does away with the objection of expense. The solution, moreover, does not require to be renewed, for by twenty-four hours the impregnation with perosmic acid is completed. I do not recommend using the solution more than once. A quantity sufficient to cover the sections will generally be found enough, but I always allow a considerable excess.

CASE OF BRAIN TUMOUR WITH AUTOPSY.[1]

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THE chief interest in this case lies in the question whether operation might have been successfully performed This question has become of vital moment since the reports of successful removal of extensive infiltrating gliomata, especially those of Beevor and of Wood.

Case.—Mr. H——, of Salem, a patient of Dr. Phippen, was seen by me in consultation with Dr. Phippen and Dr. Johnson, October 29, 1894. The history up to that time was as follows: He was well up to the preceding July, when he complained of difficulty in figuring and trouble with the trial balance. He seemed indifferent about his vacation, and said that he did not feel well. Those in the office think that the symptoms had been coming on for six months. He was a man 53 years of age, of sedentary occupation.

September 12.—An attack with loss of speech and twitching of the face occurred, lasting perhaps ten or fifteen minutes. It was not noted on which side the spasm occurred. This attack was followed by indistinctness of speech and apparently by some aphasia. On September 13, a queer sensation in the throat suddenly appeared, lasting only a minute. On September 19, he had an attack of loss of speech. This was quickly over, but for the next ten days he had a peculiar feeling in his throat.

September 30.—A severe general convulsion occurred, with a long warning. Deep coma lasted ten to fifteen minutes, followed by confusion for some hours.

[1] Shown at a Meeting of the American Neurological Association, June, 1895.

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October 9.—A convulsion occurred on the street, with some warning. His sister thinks the eyes turned to the right in this attack. He did not return again to work. The pulse was poor and the recovery slow after the deep coma of this last attack passed away, and aphasia continued up to the date of this examination.

October 14.—A slight attack with loss of consciousness occurred, with twitching in the neck and spasmodic drawing of the jaw to the right. After this attack there was repeated twitching of the right pectoralis-major muscle. The aphasia rapidly increased from this time. On October 27 the patient began to drag his right foot, and to find difficulty in holding a spoon. During the day his face became drawn to the left. The next day (October 28) he first complained of headache, indicating the frontal and left parietal regions. There was no dysphagia. The same day he had a dizzy turn in the garden. The pupils have been dilated during attacks. There has been no complaint of numbness.

Physical Examination.—The patient is not confined to his bed. He walks about with some difficulty, with toe drop on the right side. He stands very well with his eyes closed and feet together. The pupils are alike and react to light. His grasp is good on the left, but weak on the right. He cannot button his vest with the right hand. There seems to be also slight numbness in the right hand, but it is impossible to make an accurate test. There is complete loss of visual field for objects on the right (right hemianopsia). Both knee-jerks are normal. The left plantar cremaster and abdominal reflexes are lively on the left, absent on the right. The epigastric is moderate on the left, very slight on the right The tongue is deviated to the right. The nutrition is fair.

The patient appeared to understand all questions put to him, but to be unable to reply. He had the appearance of being quite intelligent, indicated the left frontal region when asked if he had headaches, opened the mouth, protruded the tongue, and shut the eyes without hesitation when requested, but when asked questions requiring an answer, hardly brought out a word, though apparently making a great effort to express himself. When asked to name objects he nodded the head, or said "yes," when the object was rightly named by another, but could not repeat, for example, the words "book" or "handkerchief." He could not say the alphabet, partly apparently through aphasia, and partly through difficult enunciation. The difficulty of testing his power of

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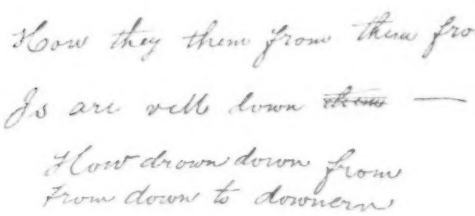
speech was somewhat complicated by the fact that he stammered previously to the onset of the disease.

A facsimile of his writing is given :—



The chirography is perfect, but sense is lacking, the writing consisting of an endless repetition of the same words. It will be noted that in two words only incorrect letters were used.

Examination of the fundus showed commencing double optic neuritis.

The comparative absence of headache, and the character of the writing, led to the consideration of general paralysis as well as of tumour. The apparent intelligence of the patient, and the comparative stability of paralysis, rendered the former diagnosis improbable, while the presence of optic neuritis, and of hemianopsia absolutely established the presence of new growth. This case illustrates the importance of testing the field of vision in case of suspected tumour of one of the hemispheres. Glioma was regarded as one of the most probable pathological diagnosis, though the patient was given the benefit of every doubt in treatment.

The question of operation was discussed, but the family were averse unless definite prospect of relief was offered. They were informed that on account of the probable size (determined by the combination, hemiplegia, aphasia and hemianopsia), and on account of the probable infiltrating character of the growth, surgical interference offered only a forlorn hope. We did not even venture to assume that the temporal lobe and angular gyrus were spared, though no word blindness nor word deafness was detected, the patient's inability to express himself increased so far the difficulty of discovering a moderate degree of these defects. In a case reported by Dr. M. H. Richardson and myself not long since,[1] the entire angular gyrus and posterior temporal region were

[1] *American Journal of Medical Science*, December, 1893.

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extensively involved in the growth, though only moderate indications of word blindness and word deafness were present.

The patient grew steadily worse, and died about one month later. Dr. Phippen removed the brain, and forwarded me the left hemisphere, which presented the following character:

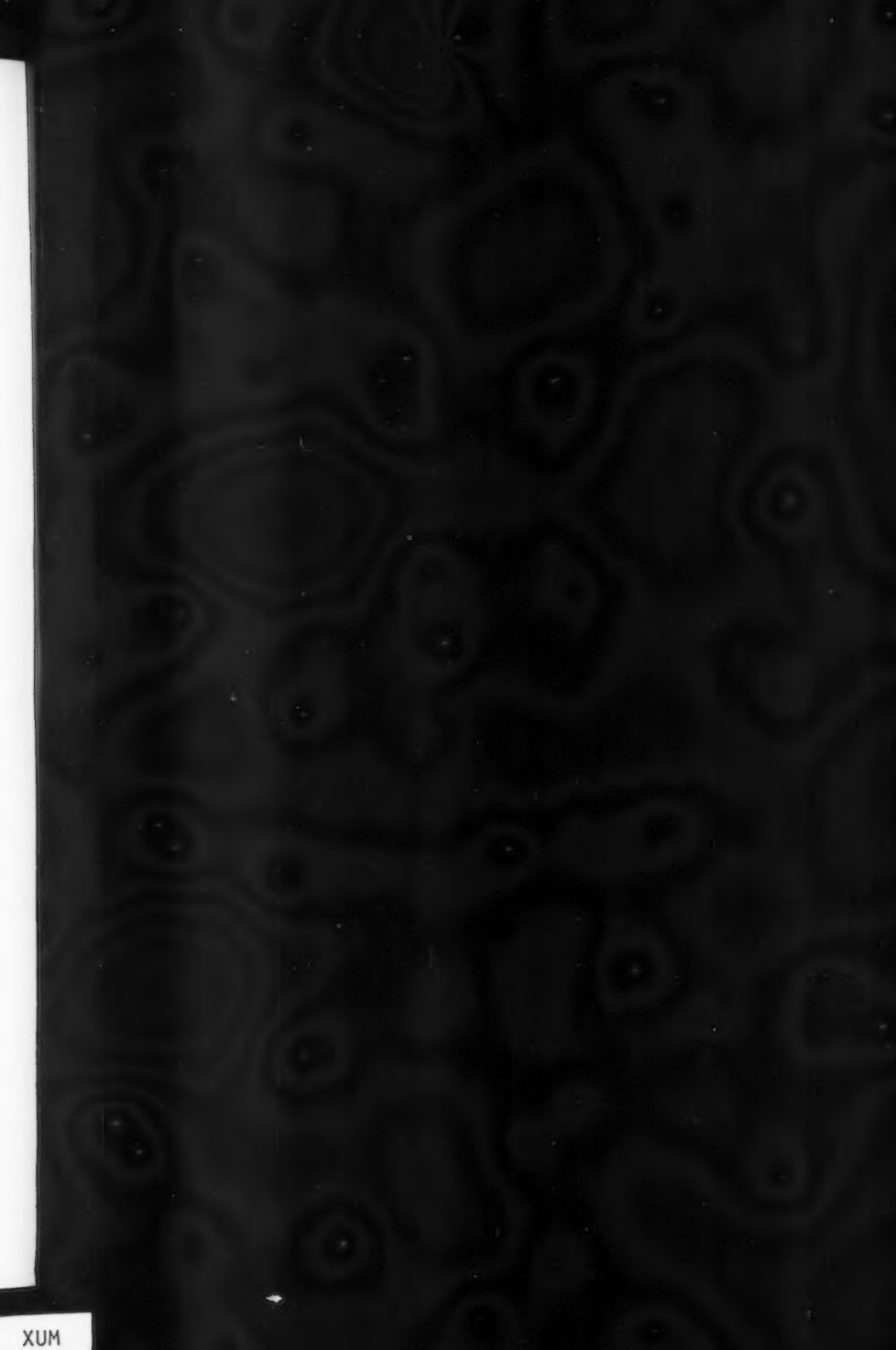
Autopsy.—The entire surface of the brain appeared normal, excepting at the junction of the Rolandic and Sylvian fissures. At this point a round tumour appeared, level with the surrounding surface, and quite sharply defined. Its consistency was approximately that of the brain substance, it was dark grey in colour. It measured two by one and a half inches. A vertical section through the growth showed a fairly sharp gradation from the tumour substance to the surrounding, apparently healthy, tissue of the centrum ovale, though no distinct capsule. Its centre was composed of white matter of softer consistency and containing hæmorrhagic spots. This was apparently the result of necrotic growth.

Section just behind the ascending portion of the interparietal sulcus showed normal brain substance. Successive sections anteriorly showed extension of the growth under the healthy cortex, beyond the transverse frontal sulcus, the termination being quite sharply marked at a point between the second and third frontal convolutions.

The following is the report of the microscopical examination by Dr. Taylor:

"The tumour given me for examination consists of a highly cellular new growth, infiltrating the surrounding brain tissue. The nuclei vary considerably in size and to a certain extent in shape, but never approach the spindle-cell variety. The transition from normal brain tissue to the pathological area is a fairly abrupt one, but without the slightest evidence at any point of encapsulation. The tumour is highly vascular, the vessels filled with blood and in certain instances containing thrombi. There is a slight amount of free hæmorrhage. In many parts of the new growth are areas of necrosis in varying stages. The tumour probably belongs to the class of gliomata.

"In the light of our present knowledge it should be stated that an absolute definite diagnosis is possible only through the use of the recently published 'Weigert-Mallory methods of differential neuroglia staining, methods which had not yet appeared when the above examination was made. There can be but little doubt that these methods are to throw the first really clear light on the many disputed points connected with the histological structure of the gliomata and distinguished from the sarcomata.'"



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* The line indicates the seat of tumour.

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The result of the autopsy, together with the fact that since that time cases have been reported of successful removal of extensive infiltrating gliomata in this region, to say nothing of a case of Dr. Putnam's (not yet reported) in which Dr. M. H. Richardson has recently removed from this locality a gliosarcomatous growth, weighing at least a pound,[1] would lead me in a case presenting similar symptoms to-day, to regard operation somewhat more hopefully than I did at that time, though experience leads us to be far from sanguine regarding the final outcome of such operations, notwithstanding individual successes, however laudable and even urgent this procedure may become as a last resort in otherwise hopeless cases.

[1] In this case the recovery, Dr. Putnam informs me, was comparatively temporary, the patient succumbing about a year later, from extension of the process.

THE DIET TREATMENT OF HEADACHE, EPILEPSY, AND MENTAL DEPRESSION.

BY ALEXANDER HAIG, M.A. AND M.D.OXON., F.R.C.P.

In several previous articles[1] I have dealt with the etiology and pathology of these troubles, and as the treatment by diet is the direct outcome of this pathology, and is now, I believe, likely to be used more extensively by others than has hitherto been the case, I propose to record shortly the more important points in my practical results up to the present time.

The above-mentioned pathology makes these troubles due to the disturbing influence of high blood pressure on the circulation of the brain. The troubles are associated, as I have shown, in a definite and distinct manner with the presence of excess of uric acid in the urine, and this is due to the simple fact that a similar and contemporaneous excess of uric acid in the blood is the direct, and probably the mechanical, cause of obstructed capillary circulation throughout the body, and so of high blood pressure.[2]

It follows from this that uric acid is the cause of all these troubles, in so far, at least, as they are not due to other obvious mechanical causes, which I need not mention; and the treatment by diet aims at clearing the blood of uric acid, and keeping it clear. Now as uric acid, or xanthin and its compounds, which are equivalent to it, are found in all kinds of fish, meat, fowl, game, their extracts and decoctions, also in eggs, and in tea, coffee, and cocoa, and similar vegetable

[1] BRAIN, Spring Numbers of 1891, 1893, and 1896.
[2] "Uric Acid as a Factor in the Causation of Disease," Third Edition, pp. 147, 172, 174, 344.

alkaloid-containing substances, all these things must be cut out of the diet. But as nitrogen is a necessary of life, these foods must be replaced by other foods of about the same nitrogen values, such as cheese and pulses; also by milk, cereal foods, and fruit, which contain little or no uric acid or xanthin.

The quantities required are not difficult to calculate, if we allow, in accordance with physiology, for an adult leading an active life 3½ grains of urea per pound, and for one who is sedentary 3 grains of urea per pound of body weight per day. Thus an adult weighing 140 pounds will require, if he is sedentary, 420 grains of urea per day, and if he is active 490 grains per day. Then, roughly speaking, urea, multiplied by 3, will give the albumen required to produce it, so that 1,260 grains of albumen would be required in the one case and 1,470 grains in the other. And as the percentages of albumen in the above foods are given in most works on food and hygiene, there is not much difficulty in calculating, with quite sufficient accuracy, how much cheese, pulses, milk, and cereal foods a given patient should have.

These preliminaries being settled, it is only necessary to add that nutrition, strength, and power of endurance should be quite as good on the one diet as on the other (as soon as the initial difficulties of taste and habit have been adjusted), and that, speaking generally, the diet does good in very many directions, and, as far as my experience goes, harm in none.

As regards the uric acid headache properly diagnosed and carefully treated by the above diet, my results have been most satisfactory. The headaches may improve at once, and, as the stores of uric acid previously in the body are eliminated, the attacks fall to one-eighth or one-tenth of their original frequency within twelve to eighteen months; the attacks also become at the same time much shorter and less severe. This improvement may set in at once almost from the day the diet is altered, or, on the other hand, the attacks may be decidedly more frequent during the first few weeks of the altered diet. It is necessary to be on one's guard against this, and to tell the patient beforehand that it is liable to occur; as, with those who do not understand its

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causation, it may have a most disheartening effect, and lead to the treatment being abandoned.

Its causation is simply as follows. The excretion of uric acid in the urine is, other things equal, dependent upon its solubility in the blood, and its solubility is dependent on the alkalinity of the blood. With high urea and good nutrition the blood is less alkaline than with low urea and feeble nutrition. It follows from this that in conditions of low nutrition the blood will be flooded with uric acid from all the places of deposit in the body in which it has been previously stored. Therefore the blood will be flooded with uric acid if, in changing diet, the patient, either from habit as to the old diet, or positive dislike of the new diet, or simple want of appetite, takes it badly, and lets nutrition down.

Then, again, many vegetable foods introduce more alkali and less acid into the body than did the animal foods left off, and this is another cause for increased alkalinity of the blood and its consequent flooding with uric acid. From one of these causes, or both combined, the headaches may be decidedly worse during the first few weeks of treatment; but this is by no means an unfavourable sign as regards ultimate results, and is only one more proof, if any is needed, that the functional trouble is absolutely dependent on uric acid.

Where these headaches are sufficiently frequent and severe to call for treatment, I generally give a mixture containing bromide of ammonium and salicylate of ammonium, to be taken for a few days, from time to time as may be necessary to help them over the first rush of uric acid.

The same will apply almost word for word to epilepsy and mental depression, in so far as they are due to uric acid.

In mental depression there is almost always some bodily depression as well, and even if there is no dyspepsia the appetite is very poor indeed. Under these circumstances, a sudden change of diet is almost certain to produce some failure of nutrition, and the depression will be worse instead of better at the first start. Here it is very little use to pre-

scribe a diet, and leave it to the patient and friends to carry it out to the best of their ability, which is often very small.

I believe that the best plan is to put the patient to bed, so as to economise force when there is none coming in; to feed by rule carried out by a trained nurse, and to increase the quantities and add massage and passive movements as nutrition improves till sufficient force has been accumulated to allow of his being up and about once more.

Such mental depression has often seemed to me to be uric acid headache, with absence of appetite and failure of nutrition; nutrition being so bad that blood pressure falls (from heart failure) below the point at which headache is possible, and depression then takes its place; feed up such patients on meat and wine diet they will, perhaps, have a good bilious attack—headache, vomiting and all—and then be fairly well for a time; but they are now storing uric acid and are certain to relapse later on.

With mental depression, also, I have got many very good results, and most of my headache cases have suffered also from depression, and the two have cleared up together. Mental depression, however, rather tends to linger after the headache, and is apt to give trouble from time to time if nutrition is allowed to fail, especially in hot weather (spring and summer).

I have purposely left epilepsy till the last, because my results with it have been not nearly so good as in the case of the other troubles, and because it presents many serious difficulties to those who would attempt to estimate the real effects of any treatment. Of late years I have prescribed the diet for a considerable number of out-patients, and have got many reports of very decided improvement as to number, and still more as to severity of the fits. On the other hand, cases treated even for many weeks in hospital, and with great care outside, as to regulation of diet, have given only most disappointing results.

In some cases of epilepsy, just as in headache, the attacks may be more frequent and severe soon after the diet is altered. I was especially struck with this in the case of a boy who was under my care at the Metropolitan

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Hospital a little more than a year ago. When he came under my care the friends were getting desperate, and ready to try anything; I accordingly put him on a very strict diet, and kept him in bed. He had previously been under observation for many weeks, and his fits had been frequent, generally two or three each day; then on my diet they soon became distinctly both more frequent and more severe, so that the resident medical officer and the sister of the ward became decidedly anxious about him, and a few doses of bromide were given, but otherwise he had no drugs.

However, I persevered with the diet, and in a week or two more the fits began to get less frequent and severe, falling to one a day and two in a day, and eventually a day, or even two, occurred here and there without any at all. At this point, however, he got scarlet fever, which had been introduced into the hospital, and going to one of the fever hospitals was lost sight of.

I have seen several other cases of epilepsy in which the fits, just as in this case, were distinctly worse when the diet was first begun, and my experience leads me to look on this as rather a favourable sign as to the possibility of future improvement, and one which strongly indicates perseverance with the diet.

Though my results with epilepsy cannot be called very favourable, they have been by no means entirely negative, and they furnish us, I think, as in the cases mentioned above, with one more parallel to be added to the very extensive series[1] which connect epilepsy with the uric acid headache, which is so extremely amenable to diet treatment. We must remember, however, that the uric acid headache is not, strictly speaking, cured; it is more commonly reduced from forty to fifty attacks a year down to three or four. The patient is always liable to suffer if he transgresses the diet, and he may suffer at any time after exposure to great cold or any unfavourable circumstance affecting importantly the solubility of the uric acid, which he still always has with him, though in lessened quantity. Regarded from this point

[1] See BRAIN, Spring Number, 1896.

of view, the results in some cases of epilepsy are not so very far behind, and I believe that in future, with more care and strictness in carrying out the diet, and in selecting early and suitable cases, we may hope for a considerable number of decided improvements, and here and there some cures, comparable at least to those of headache.

There is one other trouble due to excess of uric acid in the blood, which presents us with an exact parallel in its reaction to diet. I refer to cases of anæmia and chlorosis, and I have for several years been pointing out that my own blood actually alters its quality from day to day, in accordance with the amount of uric acid passing through it.

In cases of anæmia put on diet for headache or depression there is very often a fall in the value of the blood decimal during the first few weeks of treatment, followed by a rise; and at the end of six months, and still more at the end of twelve, it is considerably (often as much as 20 to 30 per cent.) above the point at which it stood when the diet was begun.

I mention this here because the blood is easily examined, and I believe its alterations afford us a very reliable index of the quantity of uric acid passing through it. In my own case the correspondence with daily observations of blood and urine is so absurdly close, that I often think I might give up estimating the excretion of uric acid in the urine, and trust to the changes in the blood to tell me whether it has gone up or down. An examination of the blood at the time the diet treatment is begun, and again after one month, three months, six, nine, and twelve months, will give you a very fair idea of the amount of uric acid that has been passing through the blood, and the effects of the diet upon it. And, speaking generally, if the blood decimal falls at the end of the first month it will probably have risen by the end of the third, and still more at the end of the year. Any very decided fall in the early weeks is an indication to look carefully after nutrition, and perhaps to give tonics to moderate a little the rush of uric acid.

The above facts, by the way, probably explain the common treatment of anæmia with excess of animal food,

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because the primary fall of the blood decimal on other diets frightens those who do not understand its meaning and causation. But here again, just as in the diet treatment of headache, epilepsy and mental depression, the only road to complete success may run through this belt of apparent failure.

Reviews and Abstracts.

The Present Evolution of Man. By G. Archdall Reid. Chapman and Hall, 1896. Demy 8vo., pp. 370.

Reflex Action, Instinct and Reason.

The old time controversy as to whether animals can reason as long been settled in the affirmative, but the preliminary question of what the difference is between instinct and reason still remains an unsolved problem. Mr. Archdall Reid's offer of a solution in his recent book on Psychology appears to me unacceptable, because the basis of his definitions is unsound, their form unsatisfactory, and they do not stand the test of application to actual cases.

Mr. Reid defines instinct as "the faculty which is concerned in the conscious adaptation of means to ends, by virtue of inborn inherited knowledge and ways of thinking and acting," and reason as "the faculty which is concerned in the conscious adaptation of means to ends by virtue of acquired non-inherited knowledge and ways of thinking and acting." To regard instinct and reason as "faculties"—as separate entities—is a lingering remnant of the discarded "metaphysical" mode of treating psychological questions, which is out of harmony with current methods of thought, and is a cause of inevitable confusion. It is not possible to compare things so fundamentally different as the purely physical process of reflex action and a so-called "faculty" of reason, which is presumably mental. There is no possible basis of comparison between them. The mechanism of reflex action is as purely physical as the swing of a pendulum. It is no more possible to compare reflex action with a faculty of reason than to compare the swing of a pendulum with the working out of an equation. We may, indeed, if we can get a clear idea of what is meant by a faculty, compare the faculty of instinct with the faculty of reason, but we cannot compare either with the process of reflex action; and even if we term that process also a

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faculty, we shall be no nearer to securing a basis of comparison. Mr. Reid appears to feel this difficulty, for though his chapter is devoted to reflex action, instinct and reason, and though he defines instinct and reason, he does not attempt to define reflex action. To define instinct and reason as faculties is, it is submitted, to define the unknown by the still more unknown, and so to sin against the primary canon of definition.

What we actually observe in men and animals, and can compare upon a common basis and by a common standard, are, not faculties, but acts. While we cannot compare a reflex act with a faculty of reason or of instinct, we can very easily compare reflex acts with instinctive acts and with reasoned acts; and to these acts our definitions, like our comparisons, must be limited.

No less unsatisfactory than the basis is the form of Mr. Reid's definition. A definition to be adequate must be clear; it must be intelligible. Mr. Reids definitions are neither. Apart from the term "faculty," which conveys no clear connotation, the way in which the faculty is "concerned" in the adaptation of means to ends is left unexplained. How is the faculty concerned? The term carries the implication that there are other factors also "concerned" in the process. If so, what are these factors? If not, the definition is faulty in not stating explicitly that instinct or reason is the faculty which *actuates* the adaptation, &c. Again, the expression "by virtue of" is by no means clear, either in itself or its connections. Is it the adaptation of means to ends which is effected "by virtue of" inborn knowledge, &c., or is it the faculty which is concerned "by virtue of" this knowledge, &c.? In either case, what is the influence which this knowledge, &c., exerts on the process? Not to appear captious, I will pass over the absence of any distinction being drawn between knowledge and ways of thinking, and will note a very serious defect in the definition of reason. The term is used in the colloquial sense in which it is commonly employed when "reason" is contrasted with "instinct," and though there is much "virtue" in the term "faculty," it appears that what Mr. Reid is really dealing with under the name of reason is a certain "way of acting." If reason is to be regarded as a faculty, in accordance with the old metaphysical method of speaking of the subject, we may pass this use of the term; but in a book on Evolution, which aims at presenting the results of modern psychology, it is expedient that the term should be used in the sense recognised among modern psychologists, as connoting a mental process. Lastly, it is extremely difficult to understand

what is meant by "a faculty which is concerned with the adaptation of means to ends by virtue of inherited . . . ways of action." Is not the adaptation of means to ends "a way of acting?" and if it be, what is the meaning of a way of acting by virtue of a way of acting? The definitions appear so faulty in form as, when critically examined, to be almost unintelligible; but let us accept them as they are, and apply their provisions to actual cases, and see whether they will stand the test of application; and in order to be quite fair, let us take the instances offered by Mr. Reid himself.

He adduces the case of the young alligator or turtle instinctively seeking the water on emerging from the egg. Is it seriously maintained that the new-born turtle or alligator has a *conscious knowledge* of the difference between land and water, of the direction in which water lies, and of the fact that progression in one direction will take him towards, and in the other direction away from water? If this is not asserted, the definition falls to the ground. If it is asserted, it can be denied, and of neither assertion nor denial can proof be adduced. It might similarly be asserted that the sunflower turns to the sun by virtue of a conscious knowledge of the sun's direction.

"By what term," says Mr. Reid, "shall we designate the action of the spider when he builds his web? Does the animal not know for what purpose he constructs it?" His definition of instinct (as the faculty which is concerned in the *conscious* adaptation of means to ends, &c.) depends upon the affirmative answer to this question. But the question cannot be certainly answered in the affirmative. We know nothing about the consciousness of the spider. We can only guess what it may be by analogy from our own consciousness, and while this analogy allows us to suppose that, on the second or third occasion of constructing a web, the spider knows the purpose for which the web is being constructed, it positively forbids us to suppose that he is so aware when, without previous experience, he constructs his first web. If we say that he must know, because he constructs the web instinctively, and instinct is the conscious adaptation of means to ends, we are arguing in a circle; and there is no other ground for the statement. It is evident that when the factor of consciousness is brought into the definition, a factor is brought in of which we are almost entirely ignorant, a factor about which contradictory assertions may be made without the possibility of either being proved or refuted, and, therefore, a factor which renders the definition valueless. Mr. Reid compares the action of the spider in running

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along a thread to secure his prey with that of a man running to secure a snared bird; and that of the former cutting loose a dangerous captive with that of the latter in building a golden bridge for a flying enemy; and asks in what respect the actions of the spider differ from those of the man. "Only," he says, "in that the actions of the spider result from knowledge that is inborn and inherited, whereas the actions of the man result from knowledge which is neither inborn nor transmissible." In this statement there are many assumptions, and if any one of them fails, the distinction is invalid. Take two of them only—first, that the spider knows what the effect of his action will be; second, that no other difference between the acts can be found. Both are unwarranted. There is no warrant for the statement that the spider knows that the result of cutting loose a too-powerful captive will be to save his web from destruction, any more than that the ant knows, when she is hiding a pupa-sister, that she is helping to preserve the formicine body politic. We know nothing certainly about the knowledge of the spider in the circumstances, and to postulate that his act is the result of knowledge is a gratuitous assumption, opposed to what analogy we have to guide us. That there is another distinction between the acts of the spider and the man, a distinction that is not open to this objection, will presently be shown.

Precisely the same criticism applies to the stated cases of the alligator which seeks the water and snaps at an opposing walking stick; of the nesting bird; and of the bird which utters cries of warning; as compared with the man who seeks a refuge and strikes an intervening enemy; who builds a hut; and who warns his fellow of danger. Mr. Reid's distinction fails unless we assume that the alligator and the bird respectively foresee the results of their acts, and, since he brings it forward as the only one possible, it fails also if we can find another and clearer distinction.

Almost every sentence in Mr. Reid's chapter on this subject presents obvious points for criticism, but to notice them all would be tedious. It is necessary to notice a few which bear directly upon his definitions. "Appropriate stimulation," he says, "does not cause variations and developments in reflexes and instincts," and, again, on the same page, "instinct, which, though undeviating in its promptings, is associated with consciousness, and controlled to some extent by volition, whereby the element of choice is introduced." But if instinct is controlled to any extent by volition, and if the element of choice is introduced, then appropriate stimulation *does* cause variation in instinct. For if the element of

choice is introduced, the instinct is varied, and varied to better suit the circumstances, or what is the advantage of choice? And, if varied to better suit the circumstances, then varied in obedience to the "appropriate stimulation" which the circumstances furnish.

"There is no vestige of proof that instincts are increased by stimulation, *i.e.*, that they are not only called into activity by stimulation, but sharpened by it." On the contrary, there is abundant proof that the bird with practice makes a better nest; the spider with practice makes a more perfect web. But Mr. Reid says this improvement is no longer instinct; it is reason. Then, we ask, what becomes of the absolute distinction between instinct and reason? "Instinct does not merge into reason, but is sharply divided from it. Reflex action, instinct and reason are not derived the one from any other, but each one is distinct from, and has arisen independently of, the others—is not a more or less complex form of the others." "Instinct and reason do not merge at any point."

Mr. Reid quotes Spencer as regarding instinct as arising out of reflex action, and being evolved into intelligence, and Lewes as regarding instinct as the result of intelligence, and sets them off the one against the other, and disagrees with both. No doubt, as he puts the case, the two positions are irreconcilable; and if instinct arises out of reason it seems impossible that reason can arise out of instinct; but if we alter the wording very slightly it will appear not at all inconsistent to speak of reason arising out of instinct, and sinking into instinct again.

It appears evident that such a mass of inconsistencies and contradictions in the argument points directly to a fundamental fault in the definitions upon which the argument is based; and the test of their application to actual cases confirms the impression derived from the ambiguity of their form and the unsoundness of their basis—that they do not fulfil the requirements of scientific definitions, and do not define the subjects to which they are applied.

It has been pointed out that there is no common basis for comparison between a purely physical process such as reflex action, a quasi-psychological "faculty" such as instinct, and a psychological "faculty" such as reason. By reflex action is meant the physical reaction of a living body to physical stimulation. Reason is employed sometimes to connote purely mental processes, which can be observed by introspection of our own minds only; sometimes to imply the combination of this mental process with other mental processes and with conduct. It is in this latter sense that it is

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understood when used in contradistinction to instinct, but the mischief is that, when so employed, the other more restricted meaning is continually creeping in and vitiating the argument. In whichever way it is employed, it can no more be compared with reflex action than a resolution passed at a public meeting can be compared with a sausage-machine. Before any mutually exclusive definition of any two or more things can be made, there must be some common standard of comparison to which they can all be reduced. In the present case, we can directly observe reflex action. Neither instinct nor reason can we directly observe. We cannot in any way compare reflex action with either instinct or reason, but we can very easily compare a reflex act with an instinctive act, and both with a reasoned act; and to these acts our definitions, like our comparisons, must be limited.

What, then, are the characteristics of a reflex act? If we touch the expanded tentacles of a sea-anemone they retract. They do not retract until touched, and when touched they retract. However long we watch the animal, we do not see its tentacles retract until they are touched, and when they are touched they never fail to retract. If we lightly touch the conjunctiva of a human being, the eyelid is convulsively closed. However long we watch the man, the lid is not so closed unless the conjunctiva is touched; and when the conjunctiva is lightly touched the lids never fail to close convulsively. If a foreign substance gets upon the sensitive surface of the air-passages, a cough is excited. The individual does not cough unless a foreign substance impinges upon the air-passages, and when such a substance touches the sensitive surface the cough never fails to occur. The two characters common to them are common to all reflex actions. Every reflex action is what its name implies—it is reflex. That is to say, it is the reaction of the living organism to a stimulus applied. No stimulus, no reflex action. And when the stimulus is applied it never fails to evoke that particular reaction and no other. When the tentacles of the sea-anemone are touched, they retract. They do not retract unless touched; and however often they may be touched they still retract and do nothing but retract. When the conjunctiva is touched, the lids close convulsively. However often it may be touched they still close convulsively. And the convulsive closure of the lids is the only action that is evoked by the stimulus. It is true that if a foreign body remains in contact with the conjunctiva the man may get up and go to a surgeon, but this act is manifestly no part of the reflex. So, too, however often the air-passages are stimulated by the contact of a foreign body, the cough never fails

to follow, and the action that follows is never anything but a cough. Allowance must be made for the differences in intensity of stimulus and for diffusion of stimulus. If the conjunctiva is wiped instead of being tickled, convulsive contraction of the lids does not follow; but then the stimulus is different, and the contention is that the same stimulus provokes always the same reaction. So, if the irritation of the air-passages is great and persistent, the cough will be reinforced by vomiting. But these examples evidently do not vitiate the rule. A moderate irritation of the air-passages will evoke a cough, always a cough, and nothing but a cough. A more violent irritation will evoke coughing and retching, always coughing and retching, and nothing but coughing and retching. Reflex action is, then, a reaction; it never occurs except in response to stimulus. It is unfailing; upon application of the stimulus it never fails to occur. It is unvarying; the same stimulus always evokes the same reaction. We may, therefore, sum up the nature of reflex action by defining it as an unfailing, unvarying reaction to stimulus; or, better, we may combine the two first qualities in a more comprehensive term, and define reflex action as *determinate reaction to stimulus*.

Such being the character of reflex action, how is instinctive action distinguishable from it? Like reflex action, instinctive action is determinate—that is to say, in given circumstances it is unfailing and unvarying. Under given circumstances of nutrition and warmth, the bee builds comb. Every working bee without fail takes its part in comb-building, and every bee builds comb in the same unvarying way. The garden spider never fails to make a web, and always makes a geometrical web. The trap-door spiders never fail to make their webs, and always make them to line their nests and to form a trap-door over the mouth of them. The house spiders never fail to make webs, and always make their webs in sheets and festoons. Every bird, when the nesting season comes round, makes its nest; and every species chooses a similar locality, similar materials, and a similar style of architecture. The rook always builds at the top of a tall tree, and makes its nest of live sticks. The thrush always builds in a bush, and makes its nest of grass lined with cow-dung. The kingfisher and the sandmartin always make a tunnel in the ground; the woodpecker a hole in a tree. When the season for migration comes round, the bird never fails to migrate, and migrates to the same country, the same locality, nests even in the same tree that it had sought before. When the spawning season comes round, the salmon unfailingly seeks the river. The newly-hatched turtle and alli-

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gator unfailingly seek the water; the egg-laden turtle and alligator unfailingly seek the land. The beaver never fails to make its lodge and its dam, and makes them of the same materials and in the same way as other beavers do. The rabbit always makes a burrow, the hare always sleeps in the open country, and so with all other instinctive actions. When the circumstances occur, the acts occur, and occur with unfailing, unvarying regularity. So that instinctive actions, equally with reflex actions, are *determinate*.

But they differ from reflex actions in being spontaneous. They are not reactions to stimulus. They are the natural spontaneous activity of the organism seeking expression in definite, stereotyped directions. They need no stimulus to evoke them. They occur under certain circumstances, but the circumstances are not necessary to evoke the actions, and although certain circumstances may be necessary to render the action possible, as, for instance, a running stream for the dam of the beaver, yet in the absence of the circumstances an effort to perform the action is still made, as when the beaver, confined in a room, makes in the corner a caricature of a dam out of boots and hair brushes; as when the dog who never wants a meal, buries his bones for a future need which will never arise. So with all instinctive actions; when the time for their performance comes round, the animal spontaneously attempts to perform them. If the necessary stimulus is not applied, no reflex action will take place. If the stimulus is withheld during the whole life of the organism, still the reflex action will never occur. But the instinctive action needs no stimulus. The young turtle turns spontaneously to the water. The bee spontaneously seeks its way out of the hive, seeks the flowers, and returns laden with honey and pollen. The spider needs no external stimulus to induce him to make his web. The stimulus comes from within. The action is spontaneous. So when the nesting season arrives, the bird is impelled by irresistible urging from within to set about building; and later is impelled by a similar craving to start on its weary migration. The changing season supplies the occasion, but no stimulus from without is needed. The stimulus arises from within. So the spawn-laden salmon seeks the river, the egg-laden turtle and alligator the land, urged thereto by no stimulus from without, but by a craving which arises within.

Instinctive actions, then, while they resemble reflex actions in their determinate character, differ from them in being, not reactions to stimulus, but spontaneous actions. They are *determinate spontaneous actions*. The difference between reasoned actions and

the two classes of actions that we have already considered is manifest at a glance. Reasoned acts are *indeterminate* acts. They may occur in response to stimulus, or they may arise out of the spontaneously-generated activities of the organism, but in either case they are indeterminate. They are uncertain. They vary. They are unpredictable. To take Mr. Reid's illustration: In what respect, he asks, does the action of the spider in running to secure his prey differ from that of the man running to secure a snared bird? In the origin, he says, of the knowledge by virtue of which the adaptation of means to ends is made—inherited in the one case, acquired in the other. But, apart from the uncertainty as to whether the action of the spider is prompted by knowledge at all, there is another and much more relevant difference. The action of the spider is unfailing and unvarying, undivertible, predictable with certainty. The action of the man is indeterminate. When a fly touches the spider's web, and by the action of its wings communicates a vibration to the threads, the spider rushes out. When another fly does the same thing, the spider rushes out again. If we imitate the vibration of the fly's wing by touching the web with a vibrating tuning fork, again the spider rushes out. As often as the web is made to vibrate, so often is the spider "drawn." Repeated disappointment does not check his promptness of action. The presence of a formidable enemy does not scare him into quiescence. His reaction to stimulus is strictly determinate. But the reasoned action of the man has no such determinate quality. When he has snared a sufficient number of birds, he sets his snares no more. In presence of a formidable enemy, he does not rush out to capture his birds, but remains concealed. And, in any case, when a bird is in the snare his attention may be diverted, he may be called off his task by an urgent message; he may follow some bigger game within his reach; he may wait, so as to refrain from scaring birds from a neighbouring snare. His action is indeterminate. So when a spider has a dangerous captive, he cuts him loose. However often a too powerful fly engages his web, he deals with it instantly and in precisely the same way. His action is determinate. But the man does not necessarily build a golden bridge for his flying enemy. He may prefer to attack, on the chance of exterminating him; he may hang upon his rear and harass him. His action is not determinate. So with the action of the alligator that seeks the water and snaps at an opposing walking-stick, as contrasted with the man seeking refuge and striking at an intervening adversary. To accept Mr. Reid's explanation we must assume that the alligator

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knows that he is going towards water, and knows that his action in snapping is minatory and deterrent, which, for the first occasion on which he does so, are large assumptions. The more essential distinction is that the alligator's action is determinate, the man's indeterminate. On every occasion on which the alligator is disturbed, he makes straight for the water. Even if he has previously experienced the futility of his effort at escape, the insecurity of his refuge, he still makes the same effort, seeks the same refuge. His action is certain and unvarying. But the man may seek refuge or may determine to fight; may seek one refuge rather than another; may have recourse to various stratagems; and if he has already found the refuge to be insecure he will not seek it again. The alligator attacks the foe who intervenes between him and the water. The man may attack or may avoid him. His action is variable. The same distinction may be traced between the instinctive or reflex action and the reasoned action in the other examples given in Mr. Reid's chapter.

If the distinction here drawn between reasoned acts on the one hand, and instinctive and reflex acts on the other, be the true distinction, then two consequences follow. First, that so long as any action falls completely within the definition of either, so long the distinction between them is absolute; a determinate act is instinctive or reflex, an indeterminate act is reasoned. Second, that any element of uncertainty or variability that is introduced into an instinctive or reflex act imparts into that act some element of reason; and, contrariwise, any element of certainty, or, let us say, *predictability*, that exists in a reasoned act renders that act in so far instinctive or reflex. Thus it will be seen that very few acts are purely instinctive or purely reflex, and very few acts are purely reasoned; but that the two kinds of action merge into each other by an infinite series of gradations. Nevertheless, although Mr. Reid is unquestionably wrong when he states that instinctive action is sharply divided from reasoning action, and that there is no border space where the one merges into the other, yet his statement contains an approximation to the truth; for, undoubtedly, the instinctive *factor* in any act is sharply divided from the reasoned *factor* in that act—as sharply as certainty is divided from uncertainty, and sameness from variability.

The action of breathing is adduced by Mr. Reid as an example of reflex action. According to our definition, it may more properly be regarded as instinctive, since the action is provoked, not by

stimulus from without, but by spontaneous impulse from within; but if we allow it to be a reflex action, it is still not completely determinate. It is certain that the rate of breathing will be within narrow limits, the same so long as the body is at rest in the same position; that it will be increased by exercise, greatly increased by violent exercise. But, to some extent, it is modifiable. We can at will hold our breath for a season, breathe slowly or rapidly, take deep or shallow breaths. In these cases the action of breathing, usually determinate, becomes to some extent indeterminate. An element of reason is imported into it. It becomes partly reasoned. It merges into reasoned action. Another example of reflex action is coughing, and of this the same is true. Though coughing is certainly excited by irritation of the air-passages, provided the irritation is sufficient, yet, if the irritation be only slight, the cough can be suppressed. If it cannot be wholly suppressed, it may be minimised; or it may be maximised—exaggerated. Lastly, a cough may be produced spontaneously, without the stimulus of irritation. In each of these cases the ordinarily purely reflex action may have an element of reason engrafted upon it—may become in part a reasoned action. In the last case it is purely reasoned. Yet in all cases the reflex element in the action can be sharply distinguished from the reasoned element, so that, although reflex acts may and do graduate and merge into reasoned actions, reflex action and reason remain distinct.

The action of the spider in making its web is commonly adduced as a type and example of instinctive action. Yet it is manifest that although there is a general and very striking sameness in the webs of any two spiders of the same species, no two webs, even of the same spider, are precisely alike. Each individual web has certain distinctive features, which adapt it to the particular position in which it is placed, and to the relative positions and distances of its supports. Though it is certain that an *epeira diadema* will build a geometrical web, composed, first, of main boundaries attached to supports, and enclosing an irregular polygonal space; second, of radii stretching from the centre of the polygonal space to its boundaries; and, third, of two ranges of spirals, one closely set round the centre of the space, and one more widely set external to the first; yet, when an *epeira* is placed in an enclosed space, it is quite uncertain to what points it will attach the main supports of its web, or what will be the shape of the polygonal area. Although the impulse, or, if we please, the craving, to construct the web is purely instinctive,

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and although the attachment of the spiral threads is a combination of pure instinct and pure reflex action, yet the construction of the main supports is only partly instinctive and is partly reasoned. The general action of making supports, of making them stronger than the rest of the web, and of enclosing a polygonal area within them, are instinctive. They are certain and unvarying. But since no two webs are made in precisely the same position, the particular action of attaching these supports to particular objects is a reasoned action, and an action involving sometimes a high degree of reason. Moreover, whenever reason is exhibited in the construction of a web, it is exhibited in the attachment of the supports, and not in any other part of the process. Thus the means by which the supports are attached to distant points are various, uncertain, and ingenious. The spider may hang by a thread, and allow itself to be swung by the wind to the desired spot; or it may run round, having first attached one end of a thread to the point of departure, and pull it taut on arriving at the destination; or it may float a thread in the air and allow it to be carried by the wind. In windy weather, spiders will attach the lowest part of the web to a hanging weight, instead of to a fixed object, and thus preserve their web from destruction. A better example of a reasoned action it would be difficult to find; yet it is a part of the action, considered typically instinctive, of web-spinning. In this instance, therefore, instinctive action does merge into reasoned action, that portion of the action which is certain and unvarying being instinctive, and that portion which is uncertain and varying being reasoned.

Another action usually selected as the type and example of instinctive actions is the comb-building of the bee. The domestic bee never fails to make its comb, and the comb is invariably made of hexagonal cells with pyramidal bases, composed of three rhombic plates. In an apiary of a hundred hives, each containing tens of thousands of cells, every cell answers this description. In so far, the action of the bee in building its comb is purely instinctive and reflex. But this action is subject to much variety. It is varied abundantly and continually to meet special cases, and to overcome special difficulties. Where the drone comb, built of larger cells, meets the worker comb, built of smaller cells, intermediate cells are interposed to afford a transition from the one to the other. Where a corner has to be rounded, the cells on the convex side are made with mouths larger than their bases, and those on the concave side with bases larger than their mouths. When the comb becomes heavy with its contents, its attachments

are strengthened. If a comb is torn down by its own weight, temporary buttresses are constructed from comb to comb, to hold it up until it is permanently fixed by new comb. "In places where special conditions of the situation do not otherwise permit, it may be observed that the bees, far from clinging obstinately to their plan, very well understand how to accommodate themselves to circumstances, not only in cell-building, but also in making their combs." They will pull down and rebuild repeatedly the same cell in different ways, until they have it to their satisfaction. So that the typically instinctive acts of comb-building merge into and blend with highly-reasoned acts. And the instinctive element in the action is still the determinate element, and the action is reasoned in so far as it is indeterminate.

A third typically instinctive action is the nidification of birds. The impulse to build is purely instinctive. The choice of position, of materials, of pattern, of construction, are all instinctive. That is to say, they are determinate. They are in most respects certain and unvarying, and therefore almost purely instinctive. But in some respects they are uncertain and variable, and in so far as they are thus indeterminate they lose their instinctive character and become reasoned. Thus the thrush builds its nest in a bush, and builds it of grass lined with cow-dung; in so far the action is determinate and is instinctive. But the particular bush, and the particular branch of the bush, are not determinate. These depend upon the choice of each individual bird, and of course varies with each bird, and with the same bird from year to year. That part of the action is not instinctive, it is reasoned. It is, as already said, proved beyond doubt that the first essay of the bird at nest-building is less successful, its first nest less perfectly constructed, than its subsequent nests. It learns and improves by experience and practice, and, so improving, its action varies, and thus becomes reasoned action. In this instance, again, instinctive action merges into reasoned action. An instinctive factor and a reasoned factor enter into the composition of the act, which thus becomes composite; and, although the two factors can be distinguished from one another upon analysis, yet in the doing they are combined and merged the one into the other.

The real nature and relationships of reflex action, instinctive action, and reasoned action cannot, however, be thoroughly understood without a consideration of the nervous mechanism by which they are severally actuated. Reflex action being determinate, is actuated by nervous structure which is fully and completely organised; so that the ingoing current provoked by the stimulus

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is restricted to certain definite paths, acts upon and discharges certain determinate cells, whose discharge flows through definite and restricted paths to the muscles, and produces the definite, determinate action. The process is determined by the physical conditions under which it occurs. The stimulus can no more produce a different action than water can leap over a bank. The action can no more occur without the provocation of the stimulus than gunpowder can explode without the spark. If, however, the stimulus is unusually intense, the more powerful current that it produces will evoke a more powerful discharge, which, being of too great volume to wholly escape by its accustomed channels, forces its way into neighbouring areas, and produces additional and allied actions. Thus an intense irritation of the air-passages will produce, not only cough, but vomiting.

Similarly, instinctive action is actuated by a fixed arrangement of nerve tissue, an arrangement which is inherited just as the arrangement of muscles and bones in a limb is inherited. Just as a bird inherits an arrangement of muscles attached to the bones of its wing—an arrangement such that the action of the muscles on the bones will produce the movement of flying—so it inherits a certain arrangement of nerve tissue, such that the action of these cells and fibres will cause the alternate contractions of the muscles necessary to produce the movements of flying. And so, too, it inherits a certain structure of nerve tissue such that it serves as a storehouse of energy, and, when replete, discharges the energy into the nervous apparatus which actuates the movements of flying. Thus the action of flying is spontaneous, because the nerve tissue become replete with energy; and this energy finds its way, through to channels already formed, in certain definite directions, from which the movements of flying result. The arrangement and nature of the tissues which determine this result are inherited by the bird, just as the nature and arrangement of the rest of its tissues are inherited; just as the shape of its beak, the number and form of its claws, and the colour of its feathers are inherited. The important thing to observe is, that what is inherited is not a "faculty," nor knowledge, nor a way of thinking, but an arrangement of tissues and organs.

What is true of the instinctive action of flying is true of other instinctive actions. The impulse to perform the acts arises from the repletion of certain nerve tissue with the energy which it is its function to store. The form of the action depends upon the arrangement and connections of the nerve tissue into which the liberated energy is discharged. The reason why the

repletion of tissue impels to one instinct rather than another is because the replete tissue is in relation with one set of viscera and nervo-muscular apparatus and not with another.

We have seen that, although the distinctive quality of instinctive action, as compared with reasoned action, is its determinate character; yet no instinctive action is wholly determinate. There is, in every such action, some variable factor, some element of reasoning. Even in an action to all appearance so wholly instinctive as flying or walking the direction is variable, the time of starting and arrest, the direction and speed are variable. No action, however greatly instinctive, is wholly instinctive. So, too, the arrangement of nerve tissue which underlies instinctive actions, definitely and fixedly organised though it be for the most part, yet is always to some extent plastic, modifiable, subject to disturbance and rearrangement. It is inherited with its main features, indeed, organised, but it is never completely organised throughout, or instinctive action would be as unvarying as pure reflex action.

Lastly, together with the wholly fixed, definite, stereotyped, unchangeable, completely-organised nervous arrangements that actuate reflex action, and the, in great part, fixed, and nearly completely-organised nervous arrangements that actuate instinctive action, there is inherited a quantity of nervous tissue which is still in plastic condition, still modifiable, and subject to rearrangement under the action of incident forces and of its own escaping energy. It is this portion of nervous tissue that actuates reasoned actions—actions whose indeterminate character represents the as yet incomplete organisation of the nervous arrangements to whose activity they are due. This portion of nervous tissue is, of course, inherited just as much as are the portions which actuate instinctive reflex action. The distinction is that in the last case the arrangements are inherited ready made, completely fixed; in the second they are in great part ready made, but in part subject to modification by the experience of the individual, and in the first case they are only "roughed out," and although the individual derives much assistance, and is saved much labour by the inherited tendency of this tissue to develop and organise in certain definite ways, yet, in the absence of the guidance of experience, that is, of stimulation by circumstances, it will only slowly, slightly and imperfectly so develop.

By regarding the inherited factor in conduct, not as faculty, but as arrangement of nervous tissue, we have a trustworthy guide through the perplexing tangle of the genetic relationships of the three sets of actions with which we are dealing.

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When it is stated that instinct and reason do not merge at any point, as they would were the one derived from the other, there is a confusion of thought involved in the statement, which becomes obvious when we deal, not with instinct and reason as faculties, but with instinctive action and reasoned action. Obviously an act cannot be, in the same respects, both determinate and indeterminate, and if we call the determinate portions of the act instinct, and the indeterminate portions reason, then, of course, instinct and reason do not and cannot merge into one another. But if we regard the action as a whole, and find that certain portions of it are determinate and certain other portions are indeterminate, that indeterminate action may become determinate and *vice versâ*, as in the cases examined above of the web-making of the spider, the comb-building of the bee, and the nest-building of birds; then, undoubtedly, an action may partake of the nature of both instinct and reason, and in this sense the two do most undoubtedly merge into each other; so that this argument against the origin of one from the other falls to the ground.

But, says Mr. Reid, I define instinct as depending on inheritance, reason as wholly acquired, and, since acquired traits are never inherited, instinct cannot be derived from reason; nor, since reason contains no element of inheritance save the power to be acquired, can reason be derived from instinct. If reason and instinct be regarded as faculties, and if acquired traits are never inherited, and if Mr. Reid's definitions are correct, this reasoning is no doubt sound. But if we deal with instinct and reason in terms of the acts which display them, and if we regard these acts as depending on nervous structure, we shall see reason to doubt his conclusions. Pure reflex action is actuated by a nervo-muscular mechanism, which is completely organised, that is to say, which has its parts disposed in an arrangement so fixed and determined, so shut off from the possibility of interference from without, that the action follows the stimulus as surely as the discharge of the gun follows the pull of the trigger. But it is quite in accordance with our knowledge of the arrangement of the nervous system to suppose cases in which, although the mechanism is completely organised throughout most of its extent, yet there is in some part of the circuit a breach through which modifying currents can find admission; and, correspondingly, we find that there are reflex actions, which are not purely reflex, but admit of modification and inhibition. Thus, although we cannot check or interfere with the convulsive closure of the eyes that follows a

light touch on the conjunctiva, we can modify and check the cough that follows a slight irritation of the air passages. Once allow that reflex actions can be modified—that the nervo-muscular circuit, well and deeply organised, as for the most part it is, yet in part of its course is incompletely organised, and retains some plasticity—and it is immediately apparent that the degree to which the fixity of organisation may be imperfect, and plasticity of action may be retained, is indefinite. It may be, in some cases, little or none, in others much; and hence we shall find actions, such as that of the eyelids, or the contraction of the pupils on the stimulus of light, that are purely reflex; others, like that of coughing, that are slightly modifiable; others, like that of withdrawing an injured limb, that are more modifiable, until we arrive at actions like that of undressing, or winding the watch, or crocheting when the materials are placed in the hands, or writing when a pen is put between the fingers, whose reflex character is so little apparent, is so swamped by the intelligence with which they are usually guided, that it only becomes apparent in certain morbid states of the nervous system, when the normal interference of parts of the nervous system outside of the special nervo-muscular circuit is abolished by disease.

Purely instinctive action is similarly actuated by a fixed, definite, organised arrangement of nerve tissue. But, as in the case of reflex action, the whole complexus of this arrangement is seldom organised with such completeness as to prevent the incursion, at some point, of interfering currents from without its own area. Whenever such incompleteness of organisation exists, the actions admit of modification. They become indeterminate. They partake of the nature of reason. They exhibit intelligence. Now, it is certain that incompletely organised arrangements of tissue are as freely transmitted from parent to offspring as completely organised arrangements. For instance, the majority of bones are more completely organised cartilage, yet not only bones but cartilages are inherited. Many inherited tissues are partly bone and partly cartilage, and some such tissues become in the course of life completely organised into bone. Hence there is nothing unusual, or incongruous with other instances of inheritance, in the transmission from parent to offspring of either the completely organised arrangement of tissue that actuates purely reflex action, or the well-organised, but in part incompletely organised, arrangements that actuate impure reflex action and instinct; and the point is, that not only is that part of the arrangement inherited which actuates the instinctive portion of

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the action; but that part also of the arrangement which actuates the reasoned portion of the action is likewise inherited. This latter part of the nervous arrangement is, it is true, plastic, modifiable, educable. It is inherited, not as a completely organised arrangement, but as an arrangement incipiently organised, and thus in some sense justifies Mr. Reid's description of reason that what is inherited is not reason itself, but only the power of acquiring reason. It is obvious, however, that to regard what is transmitted as nervous tissue rather than as "power of acquiring reason" is a great advance in the definiteness and clearness of knowledge.

As with reflex acts, so with instinctive acts, the proportion of unorganised or incompletely organised tissue that enters into the construction of the nervous arrangements that actuate them varies greatly in different cases, and, correspondingly, the amount of the reasoned element that is combined with the instinctive action varies, some actions being almost purely instinctive, others being almost purely reasoned, and having their motive only in instinct.

This being so, the final question, whether instinct can be evolved into reason, or whether reason can sink into instinct must be stated anew in the terms, not of faculty, but of action. It becomes the question whether reflex or instinctive action admits of modification so as to become reasoned, and whether reasoned action can become stereotyped so as to become instinctive or reflex. That the first of these questions can be answered in the affirmative we have already seen. In every instinctive action, and in nearly all reflex actions there is an element of reason. The question becomes whether this element can increase and take a larger and larger share in the action. Without adducing examples, it will be evident to those who are acquainted with the facts of animal psychology, and even from the facts already adduced, that this does occur, and that instinctive actions may and do become modified into reasoned actions.

As to the reverse change, the attainment of fixity and invariability by reasoned actions, we have the universal experience of mankind in its favour. Of the nature and mode of working of nervous processes we know little, but this fact is established beyond doubt, that nervous processes are established and organised by repetition. When a novel process occurs in the plastic, and comparatively unorganised nervous tissue, it brings about a partial and tentative re-arrangement of the tissue elements. If the process is not repeated, the new arrangement becomes partially or completely dissipated, and a mere trace, or no trace, is left.

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But if the process be repeated, the disturbed nervous elements are thereby confirmed in their new disposition; and with every repetition they become more firmly fixed, more stably compacted, more completely organised. So that it is a matter of common, of universal experience, that actions become easier by being practised. At first purely reasoned, they gradually lose their reasoned character and become more and more determinate. Although, in the lifetime of an individual, reasoned acts rarely become purely reflex, yet they often become so largely determinate that the application to them of the term "reasoned" becomes inappropriate, and they are called, in the less advanced stage "habitual," and in the more advanced stage "automatic." The response to command may become so fixed in the nervous organisation of the old soldier, that if he is suddenly called to "attention" when carrying his dinner, he will jerk his arms to his sides and let his meat and potatoes go rolling in the street. In this case, the transformation of a reasoned action into a reflex has become complete in the lifetime of a single individual. To examine the question whether the reflex thus acquired may be wholly or in part transmitted to the offspring is beside the question.

Although, therefore, we cannot refute the position that "reason and instinct do not merge into one another," since the instant an act becomes instinctive it ceases to be reasoned, and the instant it becomes reasoned it ceases to be instinctive; yet, if we deal with acts, we find that, beyond the shadow of a doubt, instinctive and reflex actions do merge into reasoned actions, and reasoned actions do merge into instinctive and into reflex actions.

CHARLES MERCIER.

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SOME WORKS ON PSYCHOLOGY.

(1) *Mental Physiology, especially in its relation to Mental Disorders.* By T. B. Hyslop, M.D., Lecturer on Mental Diseases to St. Mary's Hospital Medical School, &c. London: Churchill, 1895.

(2) *Outlines of Psychology.* By Oswald Külpe, Professor of Philosophy in the University of Würzburg. Translated by E. B. Titchener, Sage Professor of Psychology in the Cornell University. London: Sonnenschein, 1895.

(3) *Analytic Psychology.* By G. F. Stout, Fellow of St. John's College, Cambridge, &c. 2 vols. London: Sonnenschein, 1896.

(4) *An Outline of Psychology.* By E. B. Titchener, Sage Professor of Psychology in the Cornell University. New York and London: Macmillan, 1896.

The English student of psychology is to be congratulated just now on the number and variety of the text-books of the science which are appearing in his language. Some of us can remember the time when the reader was confined to the works of two writers, Herbert Spencer and Alexander Bain. Now he has a choice of English and American productions, as well as translations of German works. The publication of so many books might seem to be regrettable on the ground that the student will not know what to read. But in truth it is to be welcomed, on the one hand, as showing the greater quantity of activity now thrown into psychology, and, on the other hand, as securing the student against a one-sided and individual conception of the subject. Psychology, which deals with a multitude of diverse human minds as its subject-matter, can only be developed by the co-operation of diverse minds.

The books whose titles appear at the head of this notice have somewhat dissimilar aims, and markedly unlike characters. A word or two on these differences will help to illustrate the many-sidedness of contemporary psychological work.

Dr. Hyslop's manual of "Mental Physiology" follows, in the main, the lines of the well-known works of Dr. Maudsley and others. It is, as its title indicates, especially written for medical students. At the outset it gives a fairly full and clear account of what is known at present respecting the structure and functions of the human brain. The results of recent research into the localisation of what Dr. Hyslop not too happily calls "mental faculties" are adequately brought out. After this follow chapters on "Mind," "Sensation," "Perception," "Attention," "Memory," "Feeling," and so forth. The deviations from the normal type of mental process are dealt with in close connection with this last; and this is in many ways an advantage to the student.

The author has evidently read widely, and makes on the whole a judicious use of his material. Perhaps the most noticeable defect in the volume is the scanty reference made to the new and large field of experimental psychology. Hence, it will probably be found necessary to supplement the reading of this manual by that of Professor Ziehen "Introduction to Physiological Psychology," or some similar work.

Professor Külpe's work is an important contribution to psychology. A pupil of Wundt, he not unnaturally follows, in the main, the lines of his teacher's great work. But it is much more than an epitome of the Wundtian psychology. Külpe thinks out his subject with a good deal of fresh vigour. As a text-book it will be of real service to those who desire to be abreast of the latest results of experimental work. In this respect it is decidedly in advance of Professor Ladd's well-known works. Külpe, judiciously perhaps, omits all account of the nervous system. He everywhere assumes a knowledge of the physiological processes involved, as may be seen by a glance at his chapters on sensation. The various lines of experimental work developed at Leipzig and elsewhere are carefully summarised, and their value critically estimated. It is here that the writer is at his best. Külpe's book will long remain a valuable work of reference.

In the new treatise by Mr. Stout, the editor of *Mind*, and now Lecturer on Comparative Psychology in the University of Aberdeen, we have an examination of mental processes, written with as little reference as possible to nervous processes. It is pre-eminently a work of introspective analysis, aiming at unravelling more carefully and completely the complex processes of mind, such as perception, belief, and so forth. Here lies its value. Mr. Stout has a particularly acute eye for the subtle

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complexities of our mental life, and he has, in some important respects, carried the work of analytical psychology to a further point than had been reached by his predecessors. The writer's originality and force are illustrated in dealing, among other subjects, with that aspect of perception which he calls "the apprehension of form," *e.g.*, the rhythmic form of a series of sounds (a subject at which the Germans have of late been vigorously working), the "process of attention," and "apperception" and "apperceptive systems." In discussing this last he seeks to show that the laws of association, as ordinarily stated, do not fully explain the way in which our ideas are grouped into organic wholes. While, however, Mr. Stout keeps for the most part to the introspective point of view, he by no means despises the standpoint of physiological psychology. On the contrary, he emphasises the fact that what are called by other psychologists "psychical dispositions" are in reality psycho-physical. His use of this conception of a psycho-physical disposition is most suggestive. Other fresh and suggestive discussions concern the connection of thought and language, where he shows a careful study of the earlier forms of gesture-language, and belief and imagination. Enough has been said, perhaps, to show that Mr. Stout's work is not one to be neglected by the physiological psychologist, as written from the old "metaphysical" point of view. It is eminently scientific, both in its aim and in its method.

Professor Titchener's little manual is carefully planned and well written. He also, like Külpe, is a pupil of Wundt, and follows to some extent his teacher's mode of treatment. Yet he keeps throughout in touch with the traditional English psychology. This enables him to write clearly, and makes his manual better fitted for the average English and American student than Külpe's more elaborate text-book. Titchener, as he tells us in his preface, makes the exposition of the methods and results of experimental psychology his main object. This feature gives to the book its chief value. It may be noted that Titchener follows Külpe in his tendency to assimilate sensations and images. That is surely far enough away from the tradition of English psychology since Hume, and seems of very doubtful value. It may be added that Külpe and Titchener further agree in not recognising any simple third "quality" of mind, after sensation and feeling, answering to conation or volition. Their treatment of this subject may well be contrasted with that of Stout (book i., chap. vi., and following chapters).

JAMES SULLY.

Des Dégénérescences secondaires du Système Nerveux. Dégénérescence Wallérienne et rétrograde. By Dr. GUSTAVE DURANTE. Paris, 1895.

THE scope of this work is sufficiently indicated in the title. It consists of an extensive critical summary of the chief recorded facts bearing upon secondary degeneration, and more especially upon the so-called retrograde degeneration, the importance of which has been accentuated by the development of the "neuron theory" of Waldeyer, based upon the important histological observations of Golgi.

After a preliminary discussion of the Wallerian degeneration, illustrated by the characteristic changes following lesions of the efferent and afferent tracts in the spinal cord and the peripheral nerves, the remainder of the book is given over to examples and criticisms of retrograde alterations following various lesions. As it is unnecessary to follow the author through the numerous instances which he has given from among recorded cases and several original observations, a summary of the conclusions at which he has arrived may be sufficient.

This retrograde change, which is characterised chiefly by an attenuation of the myeline sheath of the nerve fibre with preservation of the axis-cylinder, follows not only section of the nerve, but also injuries involving the peripheral terminations of the nerve fibres. Depending upon the rapidity and intensity of the process, its appearance varies slightly. Sometimes the myeline is broken up into fine bullæ; at other times absorption takes place, so as to exhibit a progressive atrophy; changes which are clearly shown by Marchi's method. It does not invade the whole neuron simultaneously, but follows an ascending course towards the trophic centre of the nerve, which it generally, but not necessarily, implicates. The affection of these centres is not always histologically apparent, even when the degeneration passes from one neuron to another. The chief change observed here is a simple atrophy of the nerve cells without any structural alteration. For example, several instances are given of degeneration of the posterior columns of the spinal cord following cerebral lesions. The appearance presented by the posterior columns in these cases varies somewhat from that due to Wallerian degeneration in the same structures; in the retrograde form

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the degeneration is much less compact. Of similar nature to the above are the alterations observed in the spinal cord after amputation of the limbs, and numerous other examples might be cited by way of illustration.

The pathogeny of this degeneration is still obscure. Although the nerve fibre remains attached to its trophic cell, yet it is deprived of the normal impulses from adjoining structures through its peripheral terminations. The recognition of this retrograde change permits the study of a number of degenerations which were at one time believed to be of a primary nature. It shows, in fact, that the determining cause may not only be an affection of the nucleus of origin of a nerve fibre, but also of the peripheral ramification. Its acceptance indeed establishes the possibility of degeneration propagated from neuron to neuron, although a trophic centre is interposed. Such may be several forms of muscular atrophy previously regarded as of "reflex" origin; and to such also may be due the incurable nature of some peripheral paralyses, and mention has been already made of the changes in the spinal cord following amputation. It may also explain the mode of development of some forms of combined sclerosis, as well as a certain number of cases of tabes dorsalis.

There is an extensive bibliography at the end giving references to the whole subject of secondary degeneration.

W. A. Turner.

Neurologische Beiträge. Von P. J. Möbius. Heft I., "Über den Begriff der Hysterie und andere Vorwürfe vorwiegend psychologischer Art," pp. 210. Heft II., "Über Akinesia Algera. Zur Lehre von der Nervosität. Über Seelenstörungen bei Chorea." *Verlag von Ambr. Abel* (*Arthur Meiner*) in Leipzig. 1894.

The author has, in these two volumes, reprinted a number of his writings on neurological subjects; many of these papers are of much value, and are now accessible to the reader in a convenient form.

The first three papers, which deal with hysteria, have been already shortly considered in Brain, 1894, p. 142. One of them

treats of astasia-abasia. In addition the author makes some further contributions to the theory upheld by him of the psychical origin of hysteria, quoting recent observations, chiefly from the Salpêtrière School, in support of this view. With regard to hysterical anæsthesia, he agrees with Onanoff that it *may* come through suggestion; but thinks that it more commonly originates as a direct consequence of morbid brain action. The stigmata and the symptoms of the convulsions bear a similar relation to the emotions in the hysterical that their physical concomitants bear to emotions in the healthy. Just as a healthy person may turn pale and sweat from a fright, so the hysterical may become anæsthetic, or pass into a fit. Hysterical symptoms thus fall into two groups: those which are called out through some emotion, but which had a previous existence; and those directly connected with a suggestion, and which correspond to it in the form they take. One source of confusion in the literature of hysteria has been the tendency of authors to exaggerate one of these sets of phenomena at the expense of the other.

The author is in agreement with Prof. Janet's views, founded on experiment, that the most striking hysterical symptoms, especially anæsthesia, are of psychical origin, and to be assigned to a division of consciousness. He has himself expressed the same view in the aphorism that the anæsthetic hysterical patient feels but does not know it. Janet regards narrowing or restriction of the field of consciousness as the primary change, and the division of consciousness (double consciousness) as secondary to it. Möbius would regard the latter as the primary phenomenon, and points out that such a division can only be regarded as hysterical if that part which fails the waking consciousness is not entirely lost to it, but can be recovered under suitable conditions.

His own view is that the essential change in hysteria consists in that, temporarily or permanently, the psychical condition of the hysterical is similar to that of the hypnotised person. Hysteria is not, however, simply identical with increased amenability to suggestion, but is a *morbid* suggestibility. Suggestion is possible in healthy persons to a varying degree, but the resulting action corresponds to the idea suggested, whereas in the hysterical the reaction is a morbid one; painful or terrifying emotions are called out and give rise to morbid symptoms. For instance, unless hemi-anæsthesia is directly suggested, no healthy person will develop it, whereas it may occur in the hysterical, without their being in the least aware of it, as the result of a fright. Briefly, all hysterical symptoms are in form

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suggestions, but a portion of them are only indirectly due to suggestion, being a morbid reaction to emotion.

The papers immediately succeeding are occupied by an argument with Wagner as to the origin of the psychical disturbances that occur in persons revived after attempted suicide by hanging. In such cases, convulsive attacks occur, and are followed by a period of mental confusion. Amnesia is present for all events from a period a little before, to some time after, the strangulation. Wagner argues that these symptoms are to be attributed to mechanical causes, *i.e.*, sudden anæmia and subsequent great flushing of the brain with blood, due, respectively, to occlusion of the carotid and relief of the pressure. Möbius would explain them as hysterical; and in this he relies chiefly on the close resemblance clinically of the symptoms to those met with in certain phases of hysteria. This applies especially to the peculiar retro-active amnesia which occurs in precisely the same form in traumatic hysteria. This explanation Möbius does not apply to all cases, but only to those in which no structural damage to the brain has occurred. The question is dealt with at considerable length, Wagner relying on the results of experiments in animals, and on the rather meagre facts of morbid anatomy in persons that have subsequently succumbed, and Möbius on clinical observations, chiefly recent researches and methods of psychological experiment in hysteria.

Other papers in this volume deal with the value of electricity in the treatment of paralysis, and with the dangers and uses of hypnotism. In the last paper, which deals with the principles of psychology, Dr. Möbius has adopted the admirable plan of defining the psychological terms used by him, and of stating the sense in which they are used. He says that he has taken the opportunity of stating over again the opinions which form the groundwork of all his statements and the starting-point for his consideration of hysterical symptoms.

The first four papers in the second volume deal with the neurosis originally described by the author under the title of *Akinesia Algera*. The first is the original communication, of which an abstract, giving a more or less full account of the disease, will be found in BRAIN, 1894, pp. 173, 174. In the others Dr. Möbius gives the sequels of the cases previously published, with an autobiographical account of Fechner's illness, who suffered from a similar affection, also short abstracts of cases reported by other observers, which in general support his own statements, and then proceeds to a

further description of the neurosis. The patients are "degenerates," partly from hereditary, partly from acquired, conditions. He notes the frequency with which other members of the family suffer from various nerve disorders, especially milder forms of the psychoses. The exciting cause is overstrain—mental, emotional, or intellectual, the latter less commonly. The cardinal symptoms of *Akinesia Algera* are (1) the pains and other abnormal sensations which result from the performance of any action or work and (2) the consequent restriction of functional capacity. These symptoms tend to be complicated in some patients by those of neurasthenia, and in others by hysterical manifestations, and from both of these they have to be distinguished. That the pains are of psychical origin is inferred from the absence of organic lesions, and from the possibility of modifying them through psychical influences. Other symptoms besides the subjective ones and the incapacity for volitional actions are increase of the tendon reflexes, localised œdemas, and wasting of muscles. Moreover, the patients are not amenable to suggestion in any form, and, unlike normal persons, cannot be influenced by feelings of hope or fear. Affections of general sensation and of the special senses are to be regarded as secondary symptoms (hysterical). All activities, such as moving, reading, writing, seeing, hearing, and mental efforts, give rise to painful sensations in the head. But these pains, which resemble those of neurasthenia, are distinguised from the latter by their close relation to functional activity of some kind. Persistent (constant) pains in the head, apart from such activity, are at least rarely met with. One typical feature of the disease is that movements of the head itself cause no pain. Patients who are otherwise unable to make any movement can raise and move the head without trouble; similarly, movements of the oculo-motor and facial muscles are not interfered with, and the patients speak without effort or ill result. Insomnia is, as a rule, a marked feature. The sufferers thus pass into a state of absolute incapacity. (See BRAIN, *loc. cit.*)

According to the author, the prognosis is extremely bad. In only one case, that of Fechner, which differed in some respects from the ordinary type, was there a relative cure. In time a tendency to psychical disturbance, ending in insanity, shows itself. Of his own cases, one died after seven years' illness; one died insane after three years' illness; one committed suicide; and one died of tuberculosis after fifteen years' illness. Other patients observed are steadily getting worse, after illnesses of varying

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length. So far no form of treatment has been found effectual. Active treatment of any kind aggravates the disease. In time he thinks it quite possible that milder forms of the neurosis may come under observation. For the present, the author thinks it advisable to class, at any rate provisionally, *Akinesia Algera* as a distinct disease. It is most nearly allied to hysteria, and might, perhaps, be regarded as a special form of hysteria; but, on the other hand, the customary features of hysteria are absent, and the peculiar course and grouping of the symptoms make Akinesia Algera a sufficiently well defined clinical entity.

The next section of this volume consists of several papers on neurasthenia, and contains a full and most useful bibliography of the numerous writings on this disease. The author gives an interesting historical sketch of the work done on this subject. That until recently so little advance was made in our knowledge of the neuroses he attributes to the concentration of attention on the analysis of symptoms and determining of functions of organs in the light of researches into pathological anatomy and physiology, to the neglect of psychology and also of sociology, the latter arising from the limiting of medical study to the sick; whereas, if nervous diseases are to be properly understood, man must be studied in his social relations, and under normal conditions of activity. The value of a name is strikingly exemplified in the case of neurasthenia. If Beard had written on "nervousness," his observations might, like those of his predecessors on the same subject, have fallen unheeded. No doubt the increase in nervous complaints, due to the complex conditions of modern civilised life, has also been a most important factor in directing attention to them.

Dr. Möbius emphasises the need of a more exact definition of neurasthenia. He believes that clearness is gained by starting from the conception of fatigue. The symptoms of neurasthenia are those of fatigue; and the neurosis consists in an increased susceptibility to fatigue from exertion; the greater the innate tendency the slighter the exertion necessary to give rise to the symptoms. It may thus be defined as chronic fatigue, and should be spoken of, not as a disease, but as a group of symptoms.

Neurasthenia is not to be regarded as synonymous with nervousness. A man may throughout life be nervous, and have a tendency to hypochondriacal ideas, and yet show no sign of neurasthenia, *i.e.*, of abnormal fatigue from mental or bodily exertion. A sharp distinction is to be made between hysteria and neurasthenia. In hysteria the symptoms are dependent

upon idea; the essence of hysterical conditions being that ideas too easily excite abnormal changes in the organism. Hysterical states are most strikingly distinguished from neurasthenic ones by the fact that they may disappear quite suddenly, even when they have been present for a long time.

The author points out that work which involves much strain on the attention, either from its novelty or difficulty, is the kind of work which is most apt to lead to neurasthenia. In this essay there is an excellent section on treatment. The author thinks that residence at health resorts has been overdone; he divides these patients into three groups from this point of view: (1) The smallest number, who are cured and greatly improved by residence at a health resort; (2) the largest—comprising two sub-groups: the (*a*) "degenerate," and (*b*) those in whose case the exciting cause is not removed—in whom there is temporary cure, followed by a relapse; and (3) not a small group, those who return worse than they went. Therapeutic measures should be directed both to mind and to body. For the former, suggestion may be useful in a very limited number of cases; but the chief measure is the systematic guidance to a well-regulated activity, designed to call into action the organs unaffected; counsel and judicious advice as to the avoidance of injurious influences. For the body, the best methods are those which entail exercise and strengthening of the muscular system. That is the most suitable work which employs mind and body, is suited to the individual capacity, and, in the right way, alternates with sufficient rest. If possible the work enjoined should have a tangible result, otherwise it is apt to be unsatisfactory. The kind of treatment thus generally indicated is more likely to give good results than that by climate, baths, or gymnastics, which do good at first, but, as the patient becomes accustomed to them, fail of their effect.

There is also an interesting paper on the influence of heredity in nervous diseases. Several instructive family histories are recorded, especially with regard to the occurrence of neuroses and milder forms of nervous disease in the same family. Such histories show that hereditary factors play a larger part than is generally known; the more carefully such genealogies are investigated the more weight is to be attached to the milder forms of disease, and the more clearly it is seen that even the members of such families that pass as normal individuals, present evidence of hereditary taint. As regards particular points, he thinks that the moral degeneration accompanying good

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intelligence, which is described by some authors in those hereditarily nervous, is rarely to be met with.

The concluding essays deal with the psychical disturbances present in some cases of chorea.

It will thus be seen that these essays cover a wide range of subjects; and the marked ability and originality, which throughout mark their treatment, render them worthy of careful study.

J. Michell Clarke.

Manuel Pratique des méthodes d'Enseignement spéciales aux Enfants Anormaux, par les Docteurs Hamon du Fougeray et L. Couëtoux, avec une Préface du Dr. Bourneville. Paris, 1896.

The book, as Dr. Bourneville says in his preface, is addressed to all physicians, educationalists, professors, and instructors who are interested in teaching. Considerable efforts have been made by the French Government in educating normal children; but, unfortunately, the same enthusiasm has not been displayed in instructing all classes of abnormal children. This book shows that all categories of the latter class, viz., the blind, stammerers, deaf and dumb, and idiot and backward children are capable of being educated. A fair amount of good work has been done with respect to the blind, the stammerers, and the deaf and dumb; but people generally are not so convinced of the necessity of teaching idiot and backward children. Dr. Bourneville is of opinion that the special establishments for these abnormal children should receive them with less restrictions than at present, that the education should be begun as soon as possible, that as instruction is slow, parents should have as much patience as possible, that the task of education should be confided to competent persons, and that the instruction should be collective in character. When the children arrive at adult age, those who have least inproved should remain in establishments for adults, in which they can work at trades, and so diminish the cost of their maintenance; while those who have made good progress, as soon as they are discharged, should be aided by philanthropic societies, so that places may be obtained for them, in order to prevent them

from falling into habits of mendicity. It is only since the commencement of the nineteenth century, the authors say, that the question of educating the classes of children here referred to has undergone (thanks to scientific progress) its full development. This good work is, however, still unknown in great part, not only to the general, but the medical public; and the object of this book is to enlighten them on what has been done, to elicit their interest, and to gain their support. Several special treatises have been published with respect to one or other of the classes of abnormal children, but no book up to now has dealt with all classes. The work is divided into five parts: the first four deal with the study of each infirmity in particular; the fifth gives the results of teaching combinations of each defect in the same individual. Each infirmity is studied first from the physiological point of view; and the authors have endeavoured to show this as clearly as possible, and to demonstrate the physical, moral, and intellectual consequences. Then the history of each defect is described, and the methods which are in operation for alleviating it are pointed out. Chapters have been added giving statistics, lists of institutions in which special instruction is imparted, and the formalities required for admission. Lastly, the authors give a condensed account of the legislation which is applied in France to these diverse maladies. In describing the deaf and dumb, some chapters have been added on deafness, and on oral teaching, in individuals who have become deaf, but are not dumb. In the chapter on the treatment of stammering, the method employed in cases of perforation of the palate is related. The chapter on idiocy is chiefly a reproduction of a report by Dr. Bourneville to the Congress at Lyons, in 1894, on the assistance, treatment, and education of idiot children. The fifth chapter treats of the following combinations in the same individual, viz., deaf dumbness and blindness; deaf dumbness and idiocy; blindness and idiocy; deaf dumbness, blindness, and idiocy. Allusion to the well-known case of Laura Bridgman, and the less known case of Helen Keller, is made. The illustrations add considerably to the interest of a book, which will be of use to all who are concerned with the education of these abnormal children.

FLETCHER BEACH, M.B.

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Theory of the Functions in Living Matter.—By Professor E. Hering, Prague, 1888.[1]

Metabolism is, physiologically speaking, the essential distinction between living and dead matter. It signifies the chemical processes in living substance, by which, on the one hand, certain products are excreted as foreign bodies, and either accumulate *in situ*, or pass out into the circulating fluids; while, on the other, there is a simultaneous intake of nutritive matters to form new constituents. This last function is known as assimilation; the first may be termed dissimilation.

In distinguishing these functions, we must not fall into the error of regarding them as two intrinsically separate, parallel processes, and the living matter itself as a quiescent mass, used up on one side, and replaced on the other—as a copper wire dipping into copper sulphate, and traversed by an electrical current, loses copper on the one hand by decomposition, while on the other it takes up new copper. Assimilation and dissimilation must rather be conceived as two closely interwoven processes, which constitute the metabolism (unknown to us in its intrinsic nature) of the living substance, and are active in its smallest particles,—since living matter is neither permanent nor quiescent, but is in more or less constant internal motion.

To assimilate and dissimilate is a fundamental property of living matter, engrained deeply in its nature, and these functions continue—provided the essential conditions of life are present—without assistance from external stimuli; we are thus free to regard what are here termed vital conditions as being in part "internal" stimuli.

In so far as living matter is wholly unaffected by adventitious external stimuli, its assimilation (A), and dissimilation (D), may be denoted *autonomous*.

[1 Though published long ago, this very important article does not seem to have received from students of physiology the amount of attention it deserves. It has been thought desirable to make its substance known to English readers through the medium of BRAIN, but the impossibility of reducing it to an intelligible abstract has made necessary its full reproduction as a translation. —ED.]

So long as the autonomous *D* and *A* are equal, the state of the living matter cannot alter; qualitatively and quantitatively it remains the same. Such a state of perfect equilibrium between the autonomous *D* and *A* may be designated *autonomous equilibrium*.

This state of living matter is altered when any stimulus excites it to active dissimilation, no longer balanced by equal assimilation. Such dissimilation is no longer exclusively autonomous; it is reinforced by external factors, and must be denoted *allonomous*, in contradistinction from the pure autonomous process. The increased formation of *D*-products, and corresponding loss of elements that were formerly an integral part of the living matter, and included in its chemical structure, produces internal alteration in the substance, in proportion with the intensity and duration of the stimulus. Hence at the close of excitation the living matter is quantitatively and qualitatively (*infra*) altered.

If the process of dissimilation is regarded as a function of living matter, the latter must at this stage be designated less capable of functioning. Since the substance is altered, not merely qualitatively, but quantitatively also, the present as compared with the former state may be termed "below par" (*unterwerthig*). From this condition it tends to return to the earlier "at par" (*mittelwerthig*) of autonomous equilibrium. In proportion *i.e.* as living matter is excited by external stimuli to more active dissimilation, its disposition to dissimilation falls, while its disposition to assimilation rises.

Other conditions of *A* and *D* being constant, the autonomous *D* is weaker, the autonomous *A* greater, than before excitation. And living matter tends to return to its former state the more energetically, in proportion with the deficit from the preceding stimulus, *i.e.*, the amount to which it remains below par at the close of excitation. At the same time the greater disposition to *A*, and corresponding increase of assimilation, immediately after excitation, diminish with the neutralisation of the changes produced by excitation, and thus the expenditure of substance is checked again. This augments the lesser *D*-disposition, and correspondingly feeble dissimilation, until finally the *A*- and *D*-dispositions, as well as the autonomous assimilation and dissimilation, are once more equal, and the earlier "mean state" (*mittelwerthig*) and autonomous equilibrium between *D* and *A* are recovered.

Whereas this last state is characterised by equality of *A*- and *D*-dispositions in the substance, the state of deficit entails excess of *A*- over *D*-disposition.

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Obviously, therefore, the depreciation of living matter develops and proceeds *pari passu* with the impact and duration of a *D*-stimulus. The *D*-disposition falls in the same ratio, since substance below par exhibits less disposition to *D*, along with a simultaneous rise of *A*-disposition. This implies a corresponding loss of excitability towards the constant *D*-stimulus.

The excitability of living matter to *D*-stimulation, briefly *D-excitability*, thus declines in ratio with the effect of the *D*-stimulus, while the activity of the dissimilation itself is correspondingly diminished. But we have seen that the *A*-disposition (along with the simultaneous autonomous assimilation, which is not directly altered by the *D*-stimulus) rises, as *D*-excitability falls. The "down" change and expenditure of substance are thus delayed, since restoration is more vigorous than the normal; excitability to the sustained *D*-excitation also sinks more slowly. Finally, however, with constant stimulation, a new equilibrium must be reached between *A* and *D*, due on the one hand to the continuously declining *D*-excitability with declining magnitude of allonomous dissimilation, on the other to the continuously rising *A*-disposition and autonomous assimilation. From this moment, other *D*- and *A*-conditions being constant, there is no further alteration of substance; it remains at this lower level, consumption and supply being once more balanced.

Yet this reinstated equilibrium between *A* and *D* is essentially different from the state of autonomous equilibrium described above. For the balance is now maintained only through the action of the constant *D*-stimulus, which, owing to the diminution of *D*-excitability, now elicits less activity of allonomous *D* than in the former case—so that the purely autonomous *A* suffices to hold it in equilibrium. This state of equilibrium between *D* and *A*, in which the substance falls below par proportionately with the intensity of *D*-excitation, may be denoted *allonomous equilibrium*.

The down change in living matter caused by the *D*-stimulus comes, therefore, to an end, in spite of persistent excitation: a limit is reached that cannot be passed, by reason of the prevailing *A*-conditions. The substance has *adapted* itself to the constant action of the stimulus —— .

Just as we may conceive of external stimuli which compel the living substance to vigorous dissimilation, so others are conceivable which enforce greater activity of assimilation. This increase of assimilation, which is no longer purely autonomous, and is not balanced by corresponding activity of dissimilation, modifies the living matter in a direction contrary to that described above as

below par, and therefore to be denoted above par. At the close of such excitation the living matter is over-nourished; its disposition to assimilation is less than before, in proportion with the intensity and duration of the stimulus, and consequent preponderance of allonomous assimilation over autonomous dissimilation. The disposition to dissimilation is proportionately greater. Hence, at close of excitation, autonomous dissimilation preponderates over autonomous assimilation, and the living matter, owing to its gradual depreciation, returns to par.

As soon as the *A*-stimulus begins to act, a *plus* state *(ueberwerthigkeit)* of substance develops, in consequence of the preponderance of allonomous assimilation over autonomous dissimilation. This development implies depression of *A*-disposition and *A*-excitability, so that allonomous assimilation (notwithstanding constant stimulation) declines, while autonomous dissimilation increases with the increasing *D*-disposition in the substance. The disparity between the more active allonomous assimilation and less active autonomous dissimilation is accordingly lessened, and the *plus* state develops more slowly than would otherwise be the case. But since allonomous assimilation decreases steadily, while there is a constant increase of autonomous dissimilation, both must finally be equal in magnitude. The substance is now above par, and *A*-excitability is proportionately depressed, so that the constant *A*-stimulus can only effect an allonomous assimilation equal to the raised autonomous dissimilation from the rise of *D*-disposition.

Equilibrium may therefore obtain between *A* and *D* in three cases:—(1) when the living matter is at par, both *A* and *D* being purely autonomous; (2) with action of a constant *D*-stimulus, when dissimilation is allonomous, assimilation autonomous; (3) with action of a constant *A*-stimulus, when assimilation is allonomous, dissimilation autonomous. The first state is therefore *autonomous*, the other two are *allonomous* in equilibrium —— .

Descending and ascending alteration are two opposite functions, and mutually exclusive, as are also the states of below and above par which result from the 'down' or 'up' changes. The two processes of dissimilation and assimilation are also antagonistic, inasmuch as their consequences are opposite; but they are not mutually exclusive, and occur simultaneously. Other stimuli are thus conceivable which excite the living matter to an augmentation of the one process, with simultaneous reduction of the other. In proportion as such a stimulus tends to promote *D* and to reduce *A*, or *vice versa*, the down or up change in the substance

occurs the more rapidily. If, when a stimulus favourable to D and inimical to A is acting upon living matter, we could assume that the acceleration of A-disposition, concomitant with the rapid expenditure, tended to neutralise the A-depressing effect of the stimulus, the final restoration of a new allonomous equilibrium between A and D would be conceivable. But it is obvious that such a state would only be reached with a deficit much more pronounced than in the above case of the sustained action of a D-promoting stimulus. The same considerations apply to stimuli that are favourable to A and unfavourable to D.

While with stimuli that act in one direction only, *e.g.*, that are only favourable to D, the rise of autonomous A, which is a protection against too rapid expenditure, begins immediately after the commencement of the down change, along with retardation of the sinking of excitability to the persistent D-stimulus, so that the activity of allonomous dissimilation also sinks slowly; while further the new allonomous equilibrium, (by which a limit of expenditure is reached in spite of the sustained excitation) appears at a comparatively early stage of depreciation —these favourable conditions are lost so soon as a simultaneously D-promoting and A-reducing stimulus acts persistently upon the substance. Similar considerations apply to a stimulus that favours A and reduces D. This last class of stimuli, in common with that first described, induce a preponderance of dissimilation over assimilation, or *vice versa*, in living matter that is in autonomous equilibrium. Hence they might, *à fortiori*, be denoted as D- or as A-stimuli. They may further be characterised as producing an effect in both directions—in contra-distinction to the first class, which act in one direction only.

The fourth conceivable case, viz., such a modification of the living matter that A and D are simultaneously promoted or reduced, will be discussed below ——.

We are thus acquainted with two phases of alteration in living matter, descending and ascending. The former appears under the action of a D-, the latter under that of an A-stimulus, taking the state of autonomous equilibrium between A and D as point of departure. The change effected by a D-stimulus, *i.e.*, *allonomous descending alteration*, reduces the substance more and more below par. If the stimulus ceases, the living matter returns, in virtue of its internal energy, to the mean state of autonomous equilibrium. It alters, therefore, in an inverse sense to the change during action of the stimulus is, *i.e.*, ascending. This is therefore an *autonomous ascending alteration*, by which the inherent energy of the substance compensates its expenditure, and returns to par.

If, on the other hand, the living matter in autonomous equilibrium is attacked by an *A*-stimulus, the substance undergoes *allonomous ascending alteration*, and rises more and more above par. At close of excitation the substance itself sets up the opposite process, *i.e.*, *autonomous descending alteration*, and thus returns finally to the state of autonomous equilibrium.

If the living matter undergoing autonomous descending or ascending alteration is submitted to an *A*- or *D*-stimulus, the process will be more or less delayed, or even arrested (with adequate strength of stimulus), if the excitation be sufficient to produce immediate allonomous equilibrium between *D* and *A*. With a still stronger stimulus, the opposite process appears, and so forth —— .

Up to this point we have not taken into consideration that the actual magnitude of assimilation, whether purely autonomous or allonomous, depends not merely upon the actual state of the living matter and its consequent disposition to assimilation or excitability to *A*-stimuli, on the one hand, and upon the intensity of a just effective *A*-stimulus on the other; but that further assimilatory conditions must also be given, *e.g.*, a certain intrinsic temperature of the substance, presence of adequate nutritive materials, &c. It follows that when consumption of the nutritive matters exceeds supply, assimilation cannot reach the same proportions as it otherwise would, at the given *A*-disposition of substance, or strength of just effective *A*-stimulus. So, too, in dissimilation. The physiologist has grounds for assuming that an excessive accumulation of dissimilation products obstructs the functions of living matter, although it is impossible to say whether the disturbance affects the processes of dissimilation and assimilation equally, or one more than the other. It is, however, conceivable that the formation of dissimilation products may occur more rapidly than the breaking down.

Deficit of *A*-materials, or accumulation of *D*-products, may be represented on the one hand as an indirect effect of excessive *A*- or *D*-excitation; on the other, as the consequence of quite different factors, *e.g.*, altered constitution of blood, disturbance of nutritive fluids, &c.

To take the first case. Supposing the *D*-products to have accumulated in consequence of an active *D*-process, which is thus obstructed, so that the living matter, although distinctly below par, with a marked increase of *D*-disposition, cannot dissimilate to the same extent as under normal conditions.—In such a case a *D*-stimulus will fail to call out the same activity of dissimila-

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tion as under normal relations. This can only signify that the *D*-excitability of the substance no longer corresponds with the sum of the expenditure. We have thus strictly to determine between the *D*-disposition corresponding with the actual value of the living matter, and the other conditions of the normal process of dissimilation; and both the decline of the former, and any disturbance of *D*-conditions, may reduce the so-called excitability to *D*-stimuli.

A consumption of *A*-materials in excess of the simultaneous supply, with similar consequences during active *A*-excitation, is equally conceivable.

In illustration of the second case cited above,—let it be assumed that the supply of *A*-materials is from some cause defective, and no longer adequate to balance the autonomous assimilation. Autonomous dissimilation will then be maintained in the substance; but since assimilation, notwithstanding a sufficient *A*-disposition, cannot keep pace with it, the living matter undergoes descending alteration, until equilibrium with the diminished *A*-supply is finally re-established. If the *A*-supply fails altogether, while dissimilation continues, the store of *A*-materials will be quite used up, assimilation eventually ceases, and the living matter, in consequence of progressive dissimilation, undergoes radical and fatal alteration, exhausts itself, and perishes. The scantiest supply of *A*-materials, will, however, maintain a correspondingly low metabolism in the substance, as soon as equilibrium has been restored between the feeble assimilation and decreasing dissimilation. Each increment of *A*-materials will then bring back the living matter, in virtue of its augmented *A*-disposition (corresponding with the marked deficit), to energetic vital activity.

Let us further assume that the intrinsic heat essential to the normal process of metabolism cannot be maintained, from external causes, and sinks lower and lower. Then the living matter may be conceived as affected in both its processes in the same degree by this diminution of the proper heat. For in such a case dissimilation and assimilation decline, *pari passu*, without alteration of value in the substance, since the equilibrium between *D* and *A* is unaltered. And supposing dissimilation and assimilation to become minimal, the substance will not be dead, but dead *in appearance* only. Additional warmth at once restores it to its former activity.

When the depression of intrinsic heat has a more pronounced depreciatory effect upon the one process than upon the other, a new autonomous equilibrium between *A* and *D* is set up, *i.e.*, the

living matter adapts itself to the lowered intrinsic temperature in the same way as to a constant external stimulus.

The heat evolved, *e.g.*, in the process of dissimilation, must also be reckoned among the *D*-products, although not material, and must be given off again, if the substance is not to become overheated. Accumulation of heat affects the *D*- and *A*-processes equally, and is beneficial, or the reverse, according to the absolute height of the specific temperature. In this connection, much could also be said about the *internal self-adjustment of metabolism in living matter* (as already illustrated by many examples), but we must limit ourselves to a few salient instances.

We have distinguished the *D*- and *A-stimuli* which act upon dissimilation and assimilation—briefly, upon living matter—from other circumstances affecting metabolism, which we termed *D*- and *A-conditions*. This distinction is fundamental, although no absolute separation can be predicated. The external stimuli are adventitious factors; *D*- and *A*-conditions are, generally speaking, constant. As the metabolism of the living matter adapts itself to a *constant* stimulus (*supra*), so it is always adapted to what we have termed *D*- and *A*-conditions, so far as these are constant. When living substance is exposed to new *D*- or *A*-conditions—shortly speaking, to new vital conditions—these, as we have seen, act upon it at first as stimuli, until it is completely adapted to them. Thus the given intrinsic heat of any living substance may be reckoned among its vital conditions. A sudden rise or fall of internal temperature at first acts as a stimulus to living matter; but if the temperature remains constant, the substance adapts itself to it, and once this has occurred, the altered temperature ceases to be a stimulus.

It is, as we have seen, conceivable that the rise or fall of intrinsic temperature in any nervous substance may cause an equal alteration of *A* and *D*. We have already considered the simultaneous and co-extensive depression of both processes—when the value of the substance is not altered, but only the intensity of its metabolism. So, too, when *A* and *D* are reinforced in equal proportions, the living matter remains qualitatively and quantitatively the same, this is once more increased metabolism of substance, while the value is not altered.

Where, from any cause, there is a constant fall of assimilation while dissimilation continues, the latter also declines gradually, and metabolism is much reduced. The living matter then sinks more and more, and finally perishes; while if *D* and *A* are simultaneously and equally depressed, the value does not alter, and

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energy is maintained. We have, therefore, to distinguish between the value of living matter and the intensity of its metabolism at any given moment, since intensity may vary under different vital conditions, value remaining constant.

If a part of the living substance were so broken up in dissimilation that its primitive constituents were completely transformed into *D*-products, the living matter would merely alter *quantitatively*, and assimilation too would affect *quantity* of substance only. The difference in bulk between *A*-materials taken up, and *D*-products given off, in the unit of time, would not only express the quantitative increment or decrement of the living matter under investigation, but would also be the measure of descending or ascending alteration in the substance. Hence there would be merely a *waxing* or *waning* of the living matter without alteration of its internal activities; the fluctuating value would be expressed in the increasing or decreasing bulk of the substance.

But the alterations in living matter cannot be thus simply represented, nor are the phenomena of its intrinsic waxing and waning the subject of the present discussion. The dissimilatory process modifies not only bulk, but also quality of the internal activity. When the substance has been protected for a long time from external stimuli, and left to itself, the normal conditions of metabolism being present, it arrives, as we have seen, sooner or later, at the state of autonomous equilibrium between *D* and *A*, denoted above the mean state. The amount in which it is present in the tissues is in the first instance taken for granted. It is determined by the sum of all the vital conditions that constitute the environment of the substance in these tissues. If the internal movements of the constant flow of living matter could be suddenly arrested, the existing chemical relations of its primitive elements would be spatially represented in definite quantitative proportions and arrangement. Thus the absolute volume, relative bulk of the primitive elements, and actual chemical composition of the substance conceived to be in autonomous equilibrium could be determined.

If the equilibrium between *D* and *A* were in any way disturbed, *e.g.*, in favour of dissimilation (the substance thereby undergoing descending alteration), and if its movements were then suddenly brought to a standstill, we should find not merely a deficit in the absolute bulk of the substance, but alteration of its internal chemical structure also. The partial loss of primitive matters, as converted into *D* products, implies an alteration of the whole remaining complex; the substance is no longer chemically the

same,—in so far as it is legitimate to speak of chemical constitution in a substance that is in perpetual, internal, chemical movement. For not merely is there partial abstraction in dissimilation (without alteration of the permanent remainder), but the living matter also forms *D*-products internally, at the expense of that activity which characterises the mean state. And this internal, chemical, and therefore qualitative, alteration is the index of the state of deficit. Through it the substance acquires the augmented tendency to assimilation, and diminished tendency to dissimilation, denoted above as rise of *A*-disposition, and fall of *D*-disposition. If preponderance of assimilation subsequently brings the substance back to the mean state, the acquired products are once more taken up into the chemical structure, the living matter becomes again what it was, and exhibits the chemical activity characteristic of the mean state.

When the living matter in autonomous equilibrium undergoes ascending alteration, and rises above par, this is no mere quantitative increase, without change of constitution, but signifies that what is taken in, in excess of the average, enters into the chemical structure of the substance, and qualitatively alters the whole of the living matter. This is characteristic of the *plus* state, and brings about the lowered disposition to further assimilation, as well as increased disposition to dissimilation.

We have therefore to distinguish between this qualitative, as well as quantitative, up and down change, and the specific formation of new living matter (increment of substance), together with its partial destruction or consumption, which are due to pure quantitative alteration. These changes do not come into the category of the processes now under consideration. We are assuming from the outset that the living matter in the part of the organism involved is already at the maximum possible to it under the given vital conditions.

Since the actual magnitude of assimilation and dissimilation is determined, on the one hand, by the circumstances which we have designated *A*- and *D*-conditions, or conditions of metabolism in general; and, on the other, by the factors denoted as stimuli; since further, alteration of *A*- or *D*-conditions may act as a stimulus on the living matter, while a new but constant *A*- or *D*-stimulus, to which the substance has adapted itself, may also be regarded as a new *A*- or *D*-condition,—it may be asked why we should distinguish between *A*- and *D*-stimuli, and *A*- and *D*-conditions,—between allonomous, and autonomous, alteration and equilibrium of substance. Even if the distinction is accepted *ab initio* in animal

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physiology, it appeals less to the botanist. It is more particularly in regard to the nervous substance of the sense-organs, and to muscle and nerve physiology, that the distinction is called for. With no sharp line of division, the adventitious sense-stimuli are yet marked off intrinsically from other vital conditions of the excitable substance of the sense-organs; and the so-called artificial stimuli applied to the living matter of nerve and muscle, are still more distinct from the internal conditions of metabolism in these tissues.

The case is different for living substance, in which the so-called vegetative side of life predominates. The living matter of the contractile tissues, from the substrate from which our concepts of excitation, excitability, fatigue, &c., have mainly been developed, is fundamentally distinguished from most other living tissues, by the fact that the products of its chemical activity are also, generally speaking end-products of metabolism, the so-called *excreta*. This is not equally true of the living matter of the more vegetative organs in the animal organism. The products of their chemical activity are, in large part, matters which may be still further broken up in the same or other organisms, in part again such as serve immediately as *A*-materials for other living substances in the same or other animals.

We must not enter upon the question of how far the development of chemical products of vital activity in these last living substances (*e.g.*, manufacture of glycogen, fat, specific constituents of secretion, &c.), should be viewed as identical with the formation of *D*-products as described above; or if the internal chemical movements of living substance merely effect the conditions for another grouping of primitive matters,—not intrinsic to the complex of elements formed by the living tissue itself, but in direct contact with it, forming new combinations internal. In nerve and muscle substance, moreover, the parallel question recedes into the background, in comparison with the stand-point from which we have reviewed the functions of living matter. One point only claims attention. I see no reason why the pronounced development of vital energy manifested in muscular activity is to be predicated of the dissimilatory process in living matter in general. It would be easy to show that a tendency to this assumption exists in many quarters, and finds application in departments where it seems to be wholly unjustifiable.

If I have myself in this brief discussion dwelt mainly upon the processes in such living substances as it is customary to term irritable, or excitable, in the narrower sense, I am fully aware

that in other living substances, much would be prominent that is here only touched upon, or left untouched. On the other hand, the theory I have advanced seems to me broad enough to find application *mutatis mutandis* to other kinds of living matter also.

These excitable substances in the narrower sense are characterised by great lability of equilibrium, and comparatively rapid alterations of value. But since the changes in value are quantitative as well as qualitative, and the magnitude of actual *A* or *D* must be referred to quantity of substance, as standard to measure them by, we have next to seek some measure of quantity in the living substance.

When living matter has fully adapted itself to the last given *A*- and *D*-conditions (assumed to be constant), and has been sufficiently protected from the action of adventitious stimuli, the quantity of substance then present in the tissues may be taken as the standard to which to refer the intensity of the *D*- and *A*-process, could this be expressed in numbers. The quantity of *A*-matters consumed in the unit of time, referred to quantity of substance, would then measure the magnitude or intensity of the *A*-process, and the *D*-process may be similarly gauged, even where, in consequence of *A*- or *D*-excitation, the two are no longer in equilibrium, so that the substance has undergone descending or ascending alteration. The sum of *A*-matters consumed, and of resulting *D*-products, must still be referred to quantity of living matter present in the mean state, although the substance is now above or below par, *i.e.*, has increased or diminished in bulk. The same is true of the difference between *A*- and *D*-intensity, or between bulk of *A*-matters consumed, and resulting *D*-products, in the time unit; which difference, as referred to quantity in the living matter still at par, expresses *the rapidity of descending or ascending alteration* in the substance. This is the rapidity with which living matter allonomously or autonomously alters from, or returns to, the mean state. The magnitude of the whole change effected in a given time by the action of an *A*- or *D*-stimulus, in matter that was previously in autonomous equilibrium, together with the degree of *minus* or *plus* state of the substance, is, however, expressed by the difference in weight between the total consumption of *A*-materials, and the simultaneous yield of *D*-products in this time, as referred to quantity of substance in the state of autonomous equilibrium.

The quantity of living matter to which actual strength of assimilation or dissimilation is to be referred, in order to find a

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measure of the alterations due to the action of an *A*- or *D*-stimulus (for rapidity of alteration as well as for degree of resulting *minus* or *plus*) is thus a varying magnitude, inasmuch as it is dependent upon the sum of *A*- or *D*-conditions acting at the moment, and taken as constant. It follows that the degree of increase or decrease in *A*- or *D*-disposition in the living matter is also dependent upon the same conditions. But wherever the action of a stimulus upon any excitable substance is to be investigated, the inevitable point of departure must be that state of living matter which obtains after long protection from external stimuli, under otherwise normal conditions—admitting that altered conditions might bring about a different equilibrium. Whether, when the vital conditions are permanently altered, the living matter can finally become qualitatively quite the same as before, cannot here be entered upon.

In the foregoing we have briefly considered the relations that obtain between the metabolism of living matter and adventitious external stimuli on the one hand, and the so-called *A*- and *D*-conditions on the other.

A third and essential relation has not, however, been touched upon, *viz.*, the reciprocal inter-dependence of the individual elements in living matter. We know that given alterations in state at any part of the living matter can produce alterations in the state of adjacent parts in vital connection with it—alterations of state in one part may act upon adjacent parts as a stimulus. Setting aside the hypothesis that sees a merely physical process in the propagation of excitation through a nerve, the transmission of "excitation" in living matter belongs, in the first degree, to those processes which prove that alterations of metabolism in one part of the living substance may excite the same in adjacent parts. Another salient illustration of the fact that function in any individual part of a nervous organ is in coördinated relation with the simultaneous processes in other parts, is offered by the facts of simultaneous contrast. But while in those processes, which are termed transmission of excitation, the adjacent parts of the altered substance appear to undergo exactly the same kind of alteration, the living substance of the visual organ manifests another kind of interaction, *i.e.*, the alteration of one part augments the disposition to the opposite kind of alteration, or even induces the correlative change, in the adjacent parts. Many other phenomena of nerve-life illustrate the one as well as the other kind of reciprocal action between the individual parts in a larger or smaller complex of living and coherent excitable

substance. The nature of this reciprocal dependence is unknown to us; it is only discovered in its manifestations. Yet we must be on our guard, in reference to the process of conductivity, against too hasty generalisations, founded almost exclusively on observations of the motor nerve-paths to striated muscle, and of peripheral nerves alone —— .

These considerations as to function in living matter, and in nerve-substance in particular, might be held to imply for every substance *one* kind of dissimilation and assimilation only, with the corollary that living matter can only undergo in *one* kind the changes described as ascending and descending alteration. But as early as 1874, I brought forward the opposite opinion. That the *A*- and *D*-process should always be of the same, or approximately the same, nature in living matter that subserves only one kind of function in the organism, is such an obvious conjecture that I will not venture here to insist on my own theory, according to which the conducted process of excitation is by no means exactly the same in two symmetrical motor nerves of the two halves of the body. Yet I must protest against the inadequate assumption that the chemical processes representing inherent vital activity are always qualitatively[1] the same in the same nerve-fibre, or cell, or perfectly homogeneous in all nerve-fibres or cells. This theory has too long been paramount.

Johannes Müller's conception that different specific energies must be predicated for the different sensory nerves, can, in my opinion, be applied to the endless varieties of living matter in the plant and animal kingdoms. We must ascribe *specific* characteristics to the living matter of the germs of every species of plant and animal; and beyond this again, the living substance of each single germ has *individual* properties, from which proceed the intrinsic nature of its further development. It is the inherent property, the specific energy, of the living substance of the liver to secrete gall, as it is the specific energy of the living substance of the mucous gland to secrete mucin, &c. I have elsewhere expressed my general views on the question of specific energies.[1]

The activity of the gland, or the movement of the muscle-fibres bound up with the nerve, do not tell us *what* happens in the nerve-fibre, but only that *something* takes place in it. Nor do we learn much more from the nerve current, action current, and negative variation. In view of the manifold variety of the chemical pro-

[1] ("The Specific Energies of the Nervous System," Inaugural Lecture, 1882. "Lotos," v.)

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cesses which may give rise to electrical currents, we must surely hesitate to conclude from the similarity of electromotive response in two nerve-fibres (especially in cases where excitation is followed by different central and peripheral reactions), that there must be similarity of internal process in dissimilar nerves—to exclude the possibility that certain nerves may transmit different kinds of internal alteration,—or even to assume that the same process takes place in all, with the possible exception of certain sensory nerves? With regard, finally, to the central nervous organs, how can we hope to explain the control (imperfect though it be) of movements in the lower limbs, notwithstanding the destruction of the majority of conducting paths in a localised area of the spinal cord, if we exclude the possibility that the impulse descending from the brain (after crossing the surviving bridge of the spinal cord, albeit with difficulty, as an unaccustomed tract), may return to its proper path on the other side, because the latter is constitutionally predisposed to the specific quality of the passing impulse.

Muscle, gland-cells, plant-cells, perhaps indeed every living substance, exhibit electrical manifestations under given conditions, which, in their mode of appearance, present a striking analogy with the electrical phenomena in nerve: must we conclude from this that the internal chemical processes that cause these phenomena are identical in the living matter of all these parts, or that when the same electrical manifestations are to be observed in one and the same substance, in two cases, the chemical processes which underlie them must necessarily be identical?

Du Bois-Reymond's investigations of animal electricity directed general attention, in correspondence with the prevailing scientific tendencies, to the physical tokens of vital activity, and many persons deluded themselves with the hope that the intrinsic nature of nerve-activity was disclosed in the electrical phenomena of the nerve. Even optical and morphological differences in nerve-fibre were wholly subordinated to apparent identity of electromotive properties, and uniformity of electrical reaction was held to justify deductions as to uniformity of chemical constitution in nerve-substance, or at any rate, uniformity of excitatory process. According to this view there could only be one process—at least in nerve-fibre as the conducting organ, and differences of time or intensity would alone indicate possible differences of function. An exception was at most admitted for sensory nerves, but here also there was a strong tendency to attribute the so-called specific energies to qualitative differences, not in the conducting paths, but in the centres. And if there was thus a disinclination to recognise

qualitative differences in the chemical structure of different fibres, or cells, of the nervous system, and in "excitations" passing along them, still less was there any notion that multiple qualities of "excitation" might exist in the same cell or fibre. Accordingly, my proposition, that various kinds of dissimilation and assimilation may occur in the same excitable substance, so that we must distinguish between various modes of *D*- and *A*-excitation, found no response, and was even contradicted, in the first instance. As much may indeed be said for the whole of the preceding theory. Its primary application to the physiology of vision was disputed by the majority of my colleagues, the "subversal of general nerve physiology" which they professed to find in it, being regarded as "non-proven in this direction."

Even the presumption, I might say the facts, of colour-antagonism were discredited.

In the course of the year, however, individual voices began to protest against the current theory of complete homogeneity of function in all excitation of nerve fibres, and all processes of nervous conduction. With the aid of my former pupil and fellow-worker, Biedermann, I succeeded in adding to the well-known facts of general nerve and muscle physiology, which (*infra*) can be adduced in support of the above theory—new data of such an order that the presence of antagonistic conditions became still more prominent in the electromotive and excitatory phenomena of nerve, or muscle, substance also. I, therefore, allow myself to hope that the preceding considerations may now meet with a more favourable reception than in my earlier attempts to bring them forward.

Nothing essentially new has presented itself in regard to the facts in the department of visual sensation, upon which I based my theory of light and colour sense; further consideration having only deepened my conviction that the principles I then advanced require no real alteration in order to derive from them all the main facts of visual sensation; while the few points still unexplained are nowhere in contradiction with it.

Nor has extended work in nerve and muscle physiology, as well as in general biology, failed to convince me that the views I formerly expressed more briefly as to function in excitable matter are a scarcely less legitimate deduction from the facts then known than from the more striking data now before us. A hypothesis proved is no longer a hypothesis. Thus it only remains to see whether the above propositions throw light on the facts; whether they give a working plan of their interconnection; and whether they are of predictive value.

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PART II.

Modern physiology distinguishes between the state of rest and that of activity, or excitation, in excitable matter. In the terms of the theory proposed above, there could only be *one* proper state of rest for living matter, viz., that condition of autonomous equilibrium which we termed "apparent death"; because its simultaneous dissimilation and assimilation are so reduced that they may be reckoned at zero. But what the modern physiologist terms state of rest embraces not merely all the states of autonomous equilibrium, whatever the actual magnitude of assimilation and dissimilation, provided both are of equal intensity, but all those conditions also which were described above as characteristic of autonomous ascending alteration. It is true that the physiologist applies the specific name of "recovery," or restitution, to this kind of 'up' change; but the process itself is included in the general concept of rest, or non-activity, of living matter.

The different states of equilibrium between D and A may, of course, be regarded as states of comparative rest, since the living substance neither undergoes descending nor ascending alteration, but remains qualitatively and quantitatively the same. But that condition of equilibrium, which appears only when an external stimulus has acted constantly for a prolonged period, and to which the living substance has adapted itself, —denoted above the state of allonomous equilibrium—must be reckoned, in modern physiology, among the states of activity or excitation; at least, whenever the external stimulus is a D-stimulus. For it combines with the idea of general stimulation or excitation the notion of consumption, disintegration of living substances, and implies constant excitation, because an external stimulus is acting upon the substance.

The prevailing concept of excitation or activity really covers only those states or processes which we have designated as descending allonomous alteration. On the other hand, it is usual to associate the idea of excitation, or activity, with the idea of dissimilation, without reference to actual intensity of simultaneous assimilation; and many difficulties thus arise from the use of the ordinary terminology.

We have seen that if the assimilatory conditions of a nervous substance are disturbed, *e.g.*, by cutting off the blood supply, or by separating an organ from the rest of the organism, while its

D-conditions are not simultaneously affected to the same extent, autonomous descending alteration will make its appearance. But this may be indicated by symptoms which are also characteristic of allonomous descending alteration, whence we might erroneously infer that increase of dissimilation had taken place, instead of the contrary. If it may thus be assumed of any given nervous substance that it can, after stimulation, be "excited" by increase of dissimilation, as well as by decrease of assimilation, because there is descending alteration in both cases, we must distinguish between two fundamentally different kinds of stimuli as producing a 'down' change in nervous substance. In such a case it is obvious that the absolute increase of dissimilation at the excited point is not the sole factor in excitation; but that there must rather be such a disturbance of equilibrium between *D* and *A* that the former shall obtain a certain preponderance over the latter. The presumption is at least valid.

In all cases where "excitation" is not expressed by absolute increase of dissimilation, but by some symptom characteristic of descending alteration in general, the two alternatives as above must certainly be taken into consideration.

When artificial excitation of a nerve has provoked a demonstrable increase of dissimilation in the organ innervated, it is, in the first place, conceivable that the innervation acts as a *D*-stimulus on the living matter; in the second, however, that it acts as an *A*-stimulus, raising the living matter above par, and inducing augmented autonomous dissimilation. In the first case, its action on dissimilation would be direct; in the second case, indirect.

I must confine myself to these few remarks upon the different possible interpretations (as suggested by the foregoing discussion) of the same symptom of "excitation." The theoretical enumeration of every conceivable case would only be fatiguing, and what has been said suffices to show that the prevailing conception of many vital phenomena suffers from a certain onesidedness, which may easily give a warped direction to further investigation —— .

In conclusion, and to complete what was said in the first section, I will briefly point out how the essential facts of general muscle and nerve physiology can be brought under the same theoretical propositions. It was, indeed, from these facts, that the theory first developed itself. Not that it would be possible to deduce the above propositions solely from experiments on nerve and muscle fibres;—they derive from many different departments of biology, and, in particular, from the physiology of the senses, and from the general physiology of metabolism.

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When, nearly twenty years ago, these propositions were formulated from the phenomena of life—physical and psychical—the prevailing conception of the facts of general nerve physiology was strictly physical. I had, therefore, to insist, especially as against du Bois-Reymond's almost exclusively physical account of function in the nerve fibre, that these functions were essentially chemical, and that the intrinsic chemical nature of vital processes must not be overlooked in favour of their physical symptoms. Du Bois-Reymond's theory of nerve and muscle currents is purely physical. It is true that he subsequently described the supposed electromotive molecules as a definitely orientated crowd, with pronounced chemical activities; but he explained the changes of electromotivity occurring under different conditions in nerve and muscle, not by alterations in chemical activity, but by changes in the position of the molecules. My own view is, in some respects, more nearly allied to that of Hermann; but he too, in consequence chiefly of his researches into electrotonus, and polarisation in nerve and muscle, has turned more and more to a physical conception of nervous activity.

For my own part, I have only detected in the electrical manifestations of nerve and muscle, physical symptoms from which no more can be inferred as to the qualitative aspect of the functions of living matter, than from their thermic manifestations. The galvanometer, or thermometer, only express the alterations and differences of function in different parts of a living continuum, as well as the time distribution and quantitative relations of the same, and reveal nothing as to its quality.

Hermann's weighty dictum of the iso-electricity of uninjured "resting" nerve or muscle, signifies to me that such a tissue develops no current that can be led off externally, *so long as its metabolism*, i.e., *internal chemical function, is equal at all parts. Every disturbance of this equilibrium sets up currents that can be led off.* Alteration of chemical function in any part of a living continuum may, however, be expressed, not merely in its becoming *negative*, but equally in its becoming *positive* to unaltered parts. Hence, if we are to characterise the point differing in chemical function from the rest of the substance, as (relatively) altered, we must, in my opinion, *distinguish between a (relatively) positive and a (relatively) negative alteration.* And it is not altered chemical composition, but altered chemical function, which may lead to altered composition, that characterises this change. This conception thus expressed may be worked out in detail, according to the propositions developed in Part I.

We must, accordingly, distinguish, in the living matter of nerve and muscle, between ascending alteration, descending alteration, and state of equilibrium. Both ascending and descending change may occur at a very different rate, according as the intensity of assimilation, referred to the unit of substance, exceeds the intensity of simultaneous dissimilation, or *vice versâ*. If all parts of a living continuum are in equilibrium, or alter at the same rapidity, ascending or descending, there will be no current to lead off. Every difference in rate or direction of alteration, however, produces a current that can be led off (so that we may include the state of equilibrium between *A* and *D* under this general proposition, as an ascending or descending alteration at zero velocity). *We may, accordingly, conceive of all the different rates of positive or negative alteration as forming a series, of such a character that the most rapid ascending alteration forms the upper*, i.e., *positive—the most rapid descending alteration the lower*, i.e., *negative—end of the series.*

If two parts of a living continuum, which differ in chemical function, are joined by an external circuit, there will, *cæteris paribus*, be a stronger current, in proportion as the P.D. of the two points connected in circuit is greater in the above series, and the positive current will always flow through the external circuit from the point nearest the positive end of the series to that nearest the negative end. *This is the universal law of all physiological currents (vitalen Eigenströme) in nerve and muscle.*

A sartorius muscle exposed with every possible precaution, *e.g.*, one that is no longer normally nourished, will presumably undergo descending alteration (however much retarded) because dissimilation preponderates over assimilation; it is slowly dying. If the 'down' change occurs in all parts with equal rapidity, or delay, no current will be detected, even by the most sensitive galvanometer. This ideal case is never, of course, fully realised. But with a moderately sensitive galvanometer there will actually be no sign of current in such a muscle, as was conclusively shown by du Bois-Reymond (parelectronomy), and by Hermann (isoelectricity of uninjured muscle). As soon, however, as a transverse section is made in the muscle, a more rapid down change sets in at the injured point of the muscle-substance; the part immediately adjacent to the cross-section mortifies. This dead part is no longer included in the living continuum, and must be regarded as an inessential tag. The accelerated descending alteration and death, however, proceed slowly in the muscle-fibre, as may sometimes be followed directly

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with the microscope; and a more rapid down change invariably occurs at the cross-section than in the rest of the fibres. *The transverse section is, therefore, negative to the longitudinal surface of the muscle.*

So, too, with the longitudinal-transverse nerve current; and it should be remarked that the dying of the nerve as a whole is a slower process than the death of muscle; and that the assimilatory conditions of the excitable substance in excised medullated nerve are more favourable than in excised muscle.

The so-called *state of excitation in nerve or muscle* is equally characterised by descending alteration in the living matter. Each "excited" point becomes negative to each "unexcited" point, even where the whole living continuum is already undergoing a much slower descending alteration. Upon this depend the "*action-current*" (Hermann) and the "*negative variation*" (du Bois-Reymond).

"Action-currents" are, in my opinion, due to the same causes as the currents of the so-called resting nerve or muscle, in so far as both must be regarded as the external symptom of a different rate of 'down' change in the two leading-off points connected in circuit.

How far the single negative swing *(negative Einzelschwankung)* is analogous with the current-action has been sufficiently discussed by Hermann.

In my opinion, we must distinguish from the action-current in Hermann's sense (which is due to a 'down' change at one of the leading-off points) another kind of action-current, produced by an 'up' change at one lead-off, while the other is not necessarily affected in the downward direction.

Isolated muscle is quickly fatigued, and soon succumbs to "excitation," while excised medullated frog's nerve exhibits extraordinary resistance even under prolonged excitation (Wedenski, Maschek). This coincides with the more favourable conditions of autonomous assimilation in isolated medullated nerve, as indicated above. So, too, the negative variation of muscle diminishes with extraordinary rapidity on prolonged stimulation, unlike that of nerve; there may even be a *positive after variation* in the latter at the close of excitation. During excitation the longitudinal lead-off of the nerve is thrown into rhythmically recurring, rapid, descending alteration, and its value is depreciated. If it had been undergoing slow descending alteration before excitation, or was still in a state of equilibrium between D and A, it will, at close of the stimulation, be below par, and accordingly exhibits autonomous ascending

alteration, *i.e.*, becomes, in comparison with the continuous descending alteration at the transverse section, more positive than in the first instance. In excised muscle, owing to inadequate power of spontaneous recovery in the living matter, no such reaction is demonstrated after excitation; there is not even a reappearance of the original muscle-current, let alone any temporary increase of it.

The positive after-variation is most easily demonstrated on the olfactory nerve of the pike. This nerve, as Kühne discovered, exhibits marked electromotive force, since it practically consists of axis-cylinders, *i.e.*, of intrinsically excitable substance; while the accessory sheath of medullated nerves is wanting. Unfortunately, this nerve perishes more rapidly than, *e.g.*, the frog's sciatic. On leading-off the longitudinal-transverse current of a carefully prepared olfactorius, and at the same time making a rapid cut with sharp scissors near the other end of the nerve, this single stimulus will produce a negative variation, followed, as long as the nerve remains in good condition, by an unmistakable positive after-effect. I mention this experiment, with which I have long been familiar, because it does not involve introduction of any external electrical current.

If a nerve or muscle is brought longitudinally into the circuit of an electrical current, an allonomous 'up' change occurs at the point where current enters the uninjured living tissue, an allonomous 'down' change at the point of exit. This last causes the place of exit to become the starting-point of an excitation ("*closure excitation*"), *i.e.*, a down change spreading through the living continuum. If, *e.g.*, the living matter had previously been at par, and, therefore, in autonomous equilibrium between D and A, it rises above par at the point where the current enters. When the current ceases to flow, there is a corresponding autonomous down change at the point of entrance, which is the more rapid in proportion as the substance had risen above par during the previous up change. Thus, the point of entrance may become the starting-point of a second excitation ("*opening excitation*") spreading over the fibre. At the point of exit, on the contrary, there is an autonomous up change on breaking the current, provided this point has not been seriously injured by the previous action of the current, or, generally speaking, disturbed in its assimilatory conditions.

Since a rapid allonomous 'down' change occurs during the passage of current, at the point of exit, this point is negative to the rest of the fibre (in so far as the latter is not in transmitted "excitation"); while the point of entrance, in consequence of

localised allonomous 'up' change, gives the opposite reaction. This causes an internal current in the fibre, opposed in direction to the led-in "foreign" current. This internal current withers the foreign current. It has been termed a "polarisation current." But inasmuch as it is a *physiological heterodromous current*, an intrinsic vital manifestation, it must be rigorously distinguished from those polarisation currents which are not properly physiological, since they do not arise from the up or down changes in the living substance, at the points where current enters or leaves it: *i.e.*, heterodromous currents may also appear, with artificial excitation, in dead tissues, or parts that are no longer intrinsically excitable in the still living organ.

Given normal activity of living matter, an autonomous 'down' change, may, as we have seen, arise at the anode at break of the led-in, foreign current, since this point is now negative to the rest of the fibre, in so far as the latter is not undergoing progressive descending alteration; while the kathode in virtue of an autonomous 'up' change, may become positive to the rest of the fibre. In this way a physiological current is developed in the fibre, in the same direction as the previously opened foreign current. This current may be termed a *physiological homodromous*, in contradistinction to the above *physiological heterodromous* current. It appears the more certainly in proportion as the substance is more energetic; and the less the vital processes are affected by the foreign current, the more rapidly will the allonomous alterations induced by the latter (after-effect of excitation) disappear, and the opposite autonomous changes develop, when it is broken. The homodromous physiological current is more or less likely to be disturbed by complication with physical polarisation currents, of opposite direction to foreign current.

If an external current is led through the central portion of a medullated nerve, the points by which it enters and leaves the excitable matter spread far beyond the contacts of the physical electrodes. So far as these points of entrance and exit extend, there is correlatively with the distribution of the lines of current, a purely *physical* "an- and kat-electrotonus;" as may be demonstrated, *e.g.*, on a dry hollow stalk of grass without internodes, or on a bundle of the same stalks that have been lying for some time in distilled water, or weak alcohol, and are then moistened externally, and saturated internally, with salt solution.[1] From

[1] I demonstrated this experiment at the end of the sixties in Vienna. With regard to distribution of lines of current, due to the peculiar relations of conductivity in medullated nerve, I am, therefore, of the opinion to which Grünhagen finally subscribed.

this wide distribution in the excitable substance (axis-cylinder) of the nerve, of the collective points at which the external current enters and leaves it—*i.e.*, the *physiological* anode and kathode proper—those 'up' and 'down' changes develop respectively in the nerve, which are fundamental to physiological *electrotonus* (Pflüger). Both down and up change may, after closure of the foreign current, be transmitted along the nerve beyond the tracts altered in a kathodic (negative) or anodic (positive) sense by the direct action of the current, so that fugitive alterations may occur even in very remote parts of the fibre, as expressed in its electromotive reactions. On breaking the foreign current, an opposite change appears at the points of entry or exit, together with its correlative effects in the living substance, *i.e.*, an autonomous down or up change. The two points have interchanged their parts; the up change, characteristic of physiological anelectrotonus, now appears at the former point of exit, the down change significant of physiological katelectrotonus, at the former point of entrance.

In non-medullated nerve, *e.g.*, olfactorius, and in muscle, where the excitable substance, unlike medullated nerve, has no imperfectly-conducting sheath, the characteristic diffusion of entrance and exit points is wanting. The electrical phenomena which depend upon this diffusion (due, in the first place, to relations of conductivity), together with the physiological *local* consequences of the same, are accordingly absent. On the other hand, the phenomena caused by *transmission* of the up or down changes induced at the anode or kathode points of the foreign current, are more or less plainly exhibited both in non-medullated nerve and in muscle fibre.

If a tract of nerve has been traversed for some time by an external current, and the current is then reversed, the excitable matter at the point of exit (*i.e.*, former point of entrance) will be absolutely, or relatively, above par, and thus has a greater disposition to down change; the current accordingly produces a more rapid descending alteration than would otherwise be the case (Volta's alternative).

Muscle fibre, as compared with nerve fibre, has the great advantage of expressing the excitation due to descending alteration by change of form of the part affected; while a foreign current can, moreover, enter and leave by the natural ends of the fibres. With the former, the allonomous change which occurs at closure at the point of exit is, in the first instance, transmitted along the fibre, but after the closure twitch has expired it

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is confined to the vicinity of the kathode during the passage of the current (*persistent kathodic contraction*), and steadily decreases. Meantime, the autonomous up change continues at the point of entrance, and may raise the living matter considerably above par, given adequate strength and duration of current. At break, there will, therefore, be an autonomous down change, which, if sufficiently rapid, may produce an *opening twitch*, or *persistent opening contraction*, in the vicinity of the anode. Even when this autonomous down change is so weak that no visible alteration of form can be detected in the muscle, it may express itself in the physiological homodromous current (*supra*), which appears on connecting the anodic end with, *e.g.*, the centre of the muscle.

Autonomous ascending alteration cannot always be demonstrated at the kathode, on breaking the internal current, because the autonomous assimilation of the living matter in excised muscle is too slow and inadequate a process—as was pointed out above. Yet in favourable cases the autonomous upchange is exhibited in a physiological homodromous current, that makes its appearance at break, if the now kathodic end of the muscle is put in circuit with the centre.

The fact that muscle, like nerve-fibre, fails to react to transverse passage of current, obviously signifies that living matter is not identically the same living continuum in the transverse as in the longitudinal direction: as also appears from optical polarisation phenomena, and from the relations of elasticity. The failure in reaction is perhaps due to the fact that the antagonistic points at which the current leaves and enters are too closely approximated in the structural elements traversed at right angles by the current.

When a strong foreign current has been flowing longitudinally through an uninjured muscle for so long that the persistent kathodic contraction has already expired, the persistent anodic contraction (*supra*) will appear when the current is broken, and may extend over a large tract of the muscle, and last for a considerable period. If the current is then closed again, it acts as an *inhibitory stimulus* to the contracted muscle, which at once relaxes completely. The anodic stimulus of the foreign current, which tends to upward alteration in the substance, now works against the rapid autonomous down change that prevails after break at the point of entrance, and substitutes an up change. Owing, however, to the previous and exhaustive allonomous descending alteration, there will not necessarily be a renewed make contraction at the Kathode.

Just as the persistent opening contraction of a muscle may be inhibited by renewed closure of the current, so another contraction depending on autonomous down change may be inhibited by the action of an anodic current. If, just at the beginning of systole, a strong current is sent in through one brush-electrode, the point of which rests upon the frog's heart (exposed with uninterrupted circulation), while the other electrode forms contact with, *e.g.*, the skin of the throat, a more or less extended diastole of the heart-wall, starting from the point where the current enters, will make its appearance. The commencing autonomous down change is immediately converted, by the anodic action of the currrent, into an allonomous up change, and the relaxed part of the cardiac wall swells out freely in consequence of blood pressure. The contrary effect appears when current leaves the heart by the brush-electrode. If closure occurs at the beginning of the general diastole, a new systole will at once appear at the point of exit (*kathodic closure contraction*.)

If the current is left undisturbed for some time in this last direction, and is then opened during a general diastole, the wall of the heart near the brush-electrode will not take part in the ensuing systole, owing to the marked autonomous up change; it remains diastolically relaxed, and the systolic pressure of the blood causes the relaxed point to swell out considerably. This is the *kathodic opening inhibition*, which thus expresses itself in precisely the same way as the *anodic closing inhibition* above described, and cannot be viewed as a mere fatigue effect. If, on the contrary, current *enters* the wall of the heart for a prolonged period by the brush-electrode, a contraction appears immediately after it is broken, in the proximity of the Kathode. This contraction may even be more pronounced than the natural systolic contraction, as appears externally from the paler colouring of the heart-wall. This is the *anodic opening contraction* derived from autonomous descending alteration, the analogue of the *kathodic closure contraction* described above, which depends upon allonomous descending alteration.

The anodic opening contraction and kathodic opening relaxation are fundamentally analogous with the phenomena of successive contrast, as observed in other living substances, and are as little as these, to be referred to a mere fatigue effect.

Cardiac muscle is an example of the living substances which, in the state of equilibrium between D and A, vary to and fro in a regular alternation of down and up changes, so that during the period of ascending alteration the previous down change is com-

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pletely neutralised. Such a "periodically active" substance does not become exhausted, however long or short the periods, provided its assimilatory conditions remain undisturbed, and its dissimilation be not unduly increased by other influences. Within certain limits, it is capable of adapting itself to altered *A*- and *D*-conditions, or *A*- and *D*-stimuli, which may effect alterations in the period, as well as extent, of the individual changes.

Periodic "activity," in which the single up and down changes do not neutralise each other, produces an alteration in the average value of living matter, which is usually denoted as fatigue when it is descending, and is of appreciable magnitude. Tetanising excitation of the nerve also involves a periodic alternation of up and down changes; and at close of excitation an autonomous up change (positive after-variation in tetanised nerve) may appear when the substance is below par.

These indications may suffice to show the applicability to the functions of nerve and muscle substance, of the general propositions laid down at the beginning. I have laid stress on points common to both substances, since a more exact enquiry into individual questions would have to take account of the discrepancies. I am, however, far from wanting to bring all living matter under the same law, in spite of my conviction that vital function is everywhere comparable in its fundamental principles. For the rest I can only refer to the "Beiträge zur allgemeinen Nerven und Muskel Physiologie," issued from the Physiological Institute at Prague, and published in Vienna, from the year 1879. These papers are the sequel to the considerations discussed in Part I., and are, generally speaking, the extension of the same points of view, since they furnish the reader with many more examples of what was there brought forward.

F. A. Welby.

PROCEEDINGS OF THE NEUROLOGICAL SOCIETY OF LONDON.

January 14.—Dr. G. H. SAVAGE gave his inaugural address on "Heredity in the Neuroses."

March 4.—Dr. F. E. BATTEN read a paper on "The Muscle Spindle under Pathological Conditions," and illustrated his subject by means of lantern slides, photographs and sections under the microscope. Professor Sherrington and Professor Victor Horsley also made communications on Muscle Spindles, and illustrated their remarks by lantern slides and sections under the microscope. In the discussion which followed, Miss Forster, Professor Haycraft and Dr. Mott took part, and Dr. Batten replied.

April 22.—A clinical meeting was held, at which the following cases were shown :—

DR. FERRIER.—Myoclonus epilepticus. A remarkable form of nervous disease occurring in several members of one family. Two brothers, aged 16 and 13, with periodical fits, and constant irregular muscular spasms affecting almost every muscle in the body. Both became affected at the age of 7 years, as did an elder brother, who died aged 17. The disease steadily progresses, causing an ataxic spastic gait; the elder boy is now unable to stand, and the younger is about the same or a little worse than the other was three years ago.

DR. DONKIN.—(1) Probably subacute poliomyelitis. Man, aged 45 years, with paralysis and atrophy of the whole left lower extremity, supervening on an accident to the foot.

(2) Tabes with a great preponderance of sensory disturbance. Man, aged 34 years, manifold disturbances of sensibility and some loss of co-ordination in upper and lower extremities, some affection of sphincters, absent knee-jerks, doubtful wasting of some muscles of his hands, history of syphilis.

(3) Pseudo-hypertrophic paralysis in an advanced stage. Boy, aged 11 years, with paresis and wasting of some muscles of the lower extremities dating from the age of 7 years. Knee-jerks absent, electrical reactions normal.

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Dr. Gossage.—Progressive muscular atrophy commencing at the early age of 10½ years. The muscles of the left thumb were first affected, and during the next eighteen months the atrophy spread up the forearm and arm to the shoulder. The affection is limited to the one side, and has been stationary for the last 15 months.

Dr. Lunn, Dr. Beevor and Mr. Ballance.—Case of removal of cerebellar tumour. C. J., aged 49 years, admitted to St. Marylebone Infirmary, October 4, 1894. Symptoms (not relieved by antispecific remedies): Headache, purposeless vomiting, optic neuritis, weakness of right arm, right knee-jerk more brisk than left, vertigo, lateral nystagmus, tendency to fall to left, cerebellar gait. Operation, 1st stage, November 19, 1894; 2nd stage, November 26, 1894. Tumour removed was an encapsuled spindle-celled sarcoma, weighing 173 grains. It was situated at the anterior part of right lateral lobe of cerebellum. Patient recovered, but with the loss of the right eye.

Dr. James Taylor. — Syringomyelia with bulbar symptoms. Female, aged 29 years. Six months ago began to be troubled with difficulty of breathing, which has persisted. For some years had not been able to appreciate the heat of water or fire with her hands. Now marked analgesia and thermo-anæsthesia on hands and arms, slight wasting of 1st dorsal interosseus of left hand, wasting of tongue, paralysis of palate and of abductors of vocal cords.

Dr. Harris.—Man with syringomyelia. Two distal phalanges of right hand lost from whitlows, which date from 15 years ago. Analgesia and thermo-anæsthesia of arms and upper half of the body. According to the history there had been rapid wasting of left arm, with severe pain in it since a bad fall on the left shoulder four months ago, indicating a probable hæmorrhage into the cavity, if the patient's account is reliable.

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